



Institute for Empirical Research in Economics
University of Zurich

Working Paper Series
ISSN 1424-0459

Working Paper No. 90

Soft Landing of a Stock Market Bubble
An Experimental Study

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January 2002

Soft landing of a stock market bubble. An experimental study.

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January 9, 2002

Abstract

The paper investigates the effect of interest policy on price bubbles, trading behavior and portfolio choice in experimental stock markets. A series of experiments has 8 participants trade an asset over 15 periods. Alternatively, the participants can invest money in interest-bearing bonds. Treatment groups are subjected to an endogenous interest policy, while control groups experience a constant interest rate. Our stock markets are characterized by bubbles. While we observe a small positive impact of our interest policy on bubbles, the policy also strongly increases market volatility. On the other hand, concerning portfolio choice, we find evidence for value-driven (rational) investment behavior.

*The authors gratefully acknowledge feedback and comments from seminar participants at the University of Zurich, Stanford's Graduate School of Business, the Swiss National Bank, the University of Frankfurt, Bergen Business School, and of several traders of the Fixed Income Group of Goldman Sachs London; most notably of Driss Ben-Brahim, Jon Bendor, Jeremy Bulow, Ernst Fehr, Chip Heath, Tunay Tunca, Bob Wilson, and Muhamet Yildiz.

1 Introduction, literature review, and new questions

For much of the last quarter of the 20th century, it has not been en vogue to be an intervening macroeconomist - both the monetarist revolution and the efficient market hypothesis (combined with rational expectations) appear to advise against interventions of central banks. Gradually, this has been put into question, most notably after the discovery of 'excess volatility' (an extent of volatility on real stock markets that cannot be explained by standard economic models (Shiller (1981))). Subsequently, economic theory began to consider alternative approaches to understanding financial markets, such as behavioral models. Recent macroeconomic experience (e.g., the Asian market crisis in 1997) as well as new theoretical approaches to the analysis of financial markets accelerated a change in attitude, and renegade macroeconomists have gone even further and begun to talk of "the return of depression economics" (Krugman (1999)).

Alan Greenspan, chairman of the Federal Reserve Board (Fed), highly visible and influential in central bank policy, has long been a moderating voice - his opinion that markets overreacted and showed signs of "irrational exuberance" became almost proverbial. Although originally at times ridiculed, this point of view has found validation over the course of the year 2000. Using an active interest rate policy, the Fed tried to engineer a 'soft landing' of the economy, in particular of stock markets - a macroeconomic experiment on a grand scale. One of our motivations was to try to extract the essence of such a real world experiment and test it in a laboratory setting. At the very least, this is less costly and less risky, and might still give some feedback on how several important policy variables interrelate.

Our experiment naturally extends a tradition of laboratory stock market experiments. A typical laboratory stock market has the following structure. Six to eight traders interact with each other using some electronic trading system over 12 or 15 periods. Trade takes usually place using a continuous double auction, the traded asset is a stock that pays a dividend at the end of each period, and the dividend is mildly stochastic and stationary over time. In such a market, the fundamental value of the asset equals the product of the number of remaining periods times the expected value of the dividend draw. Traders begin the experiment with an endowment in stock and some experimental currency that will be exchanged at the end of all trading into dollars at a pre-specified rate. All this is common information. Although it is known that common information does not necessarily imply common knowledge because priors possibly differ, economic theory would still typically predict trading at or near the fundamental value of the stock - or no trade at all, because on a group level this is a zero sum game.

Despite all this, laboratory trading deviates in a 'bubble' pattern from fundamental value. The following stylized facts characterize a typical experimental stock market bubble (compare figure 3). Trade in early periods tends to be close to fundamental value, not infrequently even under it. People then bid the stock price up and eventually trade at prices that are significantly higher than

the fundamental value - until a certain point in time (here about period 10 or 11) after which the stock price begins to crumble, sometimes in outright crashes (fast decline in price at high trading volume).

Naturally, economists are puzzled: 'rational' people would not do this (e.g. Tirole (1982)). A possible explanation is the 'greater fool theory' (or Keynes' 'beauty contest') - even if you are aware of the inherent value of an object, you are rationally willing to pay more as long as you believe that you will find another trader to buy the asset from you at an even higher and even less 'rational' price, be that because she is a rookie or because she speculates even more aggressively. Until a short while ago, this was the hope that drove regular people, often rookies, to quitting their jobs in order to pursue full-time momentum trading (day trading).

This explanation has already been proposed by the authors of the seminal paper in the literature (Smith, Suchanek, Williams (1988)). It is all the more surprising that Lei, Noussair, Plott (2001) have debunked very convincingly the speculation motive as a sole source of laboratory stock market bubbles. Briefly, they created a stock market structure that prevented speculation, and observed very little effect on any bubble measure.

Other papers have investigated the stability of the observed phenomenon with regard to different treatment parameters. King, Smith, Williams, van Boening (1993) show that neither of the following have an impact on the occurrence or size of bubbles: the possibility to short sell stock, to buy on margin, identical endowments, transaction costs ('brokerage-fees'), professional traders as experimental subjects, nor price caps and floors. The only possibility they identify to reduce bubbles is to familiarize some participants with the results of the Smith, Suchanek, Williams (1988) paper first. Schwartz and Ang (1989) check the 'house money' hypothesis - they let people trade with their own money -, to little avail.

Smith, Suchanek, Williams (1988) also hypothesized that the described trading pattern might be due to risk-aversion in early periods (trades under fundamental value) that leads to price increases in subsequent periods which in turn create momentum; but Porter, Smith (1995) rule out risk-aversion as a major factor. Bubbles are also stable with respect to differences in market organization (van Boening, Williams, LaMaster (1993)).

Only futures markets (Porter, Smith (1995)) and the experience of subjects have been found to moderate bubbles. The latter result has been celebrated as a partial if not complete reconciliation of stock market bubble experiments with the predictions of economic theory. We have our doubts. The use of experienced subjects amounts to re-endowing rookies who got stripped of all cash on a first try, and to bringing them back to trade again with the same players that they know just ruined them. Successful traders who continue to participate in real stock markets for a long time though are professional trading houses (and some lucky individuals), and *fresh* rookies show up regularly.¹

¹ Anecdotically, Robert Wilson pointed out that practitioners estimate that the average trading rookie has perished after about six months.

Be that as it may, experimental stock market bubbles arise, and are stable with respect to virtually all market parameters. Thus the question: what to do about them? In the 'real world', the best known response is to raise key interest rates, usually by 25 base points at a time, sometimes by 50. This creates higher opportunity costs of holding stock, and is meant to directly discourage investments as well.

This paper focuses on the first idea: interest policy and opportunity costs. We introduce a portfolio alternative to trading in stocks: an interest-bearing bond. Based on an endogenous interest rate policy algorithm, we raise the interest rate in treatment groups when we observe bubbles. Control groups - unknown to them - face a fixed interest rate. We are interested in several questions. One, is it possible to influence bubbles - to reduce them based on one or some of a variety of bubble measures we propose? Two, does the portfolio choice of participants exhibit elements of rational choice, or present new puzzles? Given that no-one has examined this market structure before, we also want to thoroughly examine how our results compare to earlier experiments. We find some support for questions number one and two. Most notably though we observe a clear increase in market volatility because of our interest policy. We also find evidence against the *active participation hypothesis* (a criticism that has been occasionally raised against laboratory economics in general), i.e. the claim that bubbles arise because our participants are bored and all they can do is trade for the duration of our experiments.

Section 2 describes the experiment in more detail. Section 3 analyzes the data. We first define some bubble measures, then formulate five conjectures that we are going to check with our analysis. After a brief overview over our results, we quantify our bubble measures and other treatment variables. In order to get an idea of the percentage of fundamental (rational) trading in our experiment, we introduce a noise trading model and estimate the implied proportion of rational traders by markets. An analysis of our conjectures follows, and some conclusions are in section 4. The appendix contains further data, the experimental instructions, and the interest rate policy algorithm.

2 Experimental design and procedures

2.1 Basic design

Our experiment deals with portfolio choice of individual investors. Participants receive an initial endowment of stocks and of a fictitious experimental currency (called "Gulden"). The stock is characterized as follows:

- A finite life of 15 periods.
- A stationary random dividend payment at the end of each period of either 0, 8, 28, or 60 Gulden (for an average of 24). All payoff are equally likely.
- No redemption value at the end of the experiment, i.e. after period 15.

Realized trading gains and dividend income is immediately added to the participants' working capital and can be used, in subsequent periods, for further trade in stocks.²

Alternatively, participants can invest cash in interest-bearing bonds. The interest rate is $i = 0.05$ in the first period, but variable in principle. The interest i is paid at the end of the period. Our subjects take a portfolio decision each period. They cannot access money invested in bonds (to trade in stocks) for the rest of the respective period. The experiment thus consists of three phases:

1. Participants decide how to split their total cash for the current period. Money put into bonds bears interest, but cannot be used to trade. Money in trade accounts does not bear interest, but can be used to trade stocks in phase 2 of this period.
2. Trade in stocks takes place. Trade is organized as a continuous double auction and lasts for 150 seconds each period.
3. The dividend for this period is determined. Income from dividends on shares of stock and interest on bonds is added to the participants' total cash account, together with the current amounts in their trade and bond accounts. At the beginning of the following period, participants have money only in their *total cash accounts*, and a number of shares in their *stock accounts*.

In figure 1, we show the net present value (NPV) of the stock at an initial interest rate of $i = 0.05$. For period $t \in \{1, 2, \dots, 15\}$, the NPV is defined as

$$\text{NPV}(t) = \sum_{i=t}^{15} \frac{E[\text{dividend}_t]}{1+i},$$

using the obvious notation. Valuation of an asset using the NPV concept is standard practice and amounts to assuming risk-neutrality.³ The *upper bound value* (UBV) is the following: assume the highest possible dividend payment is drawn in each period and calculate the NPV of a stock with this certain dividend.

Previous experiments (such as Lei, Noussair, Plott (1999), Porter, Smith (1995), Smith, Suchanek, Williams (1988), or Smith, van Boening, Wellford (2000)) informed the participants in each period about the current NPV of one share of stock, to make sure that bubbles did not merely happen because of individual calculation errors. Because in their setting no interest-bearing alternative exists, the NPV is simply the sum of expected dividends. Given that

²We conducted a number of control experiments in which interest income was paid out at the end of the experiment, but was not available to participants before. While this design conforms less to reality, it allows to precisely distinguish between the income and substitution effect of the interest policy; but as it turned out, bubble sizes were very comparable.

³Assuming risk-aversion would only increase bubble sizes.

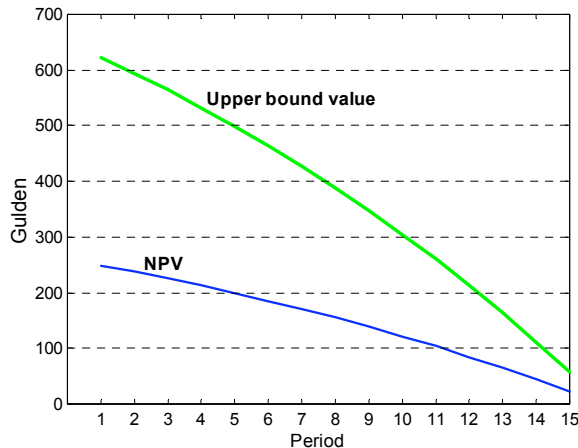


Figure 1: Net present value (NPV) of a stock (at 5 percent interest).

our experiment is more complicated (even with no change in interest rates), we cannot expect everyone to immediately understand the concept of a discounted NPV. And to explain to our participants how a change in interest rates influences the NPV would almost certainly have confused some of them.

For this reason, and because our focus was not on bubbles per se but on the impact of interest policy on bubbles, we decided to omit the periodic reports of NPVs to the participants. Instead, through careful instructions and a set of test questions, we made sure that the participants understood well the dividend draw in each period - its impact on "expected" values, and best and worst possible cases. Although several of the participating students had previously attended classes in introductory statistics, we did not rely on any mathematical or statistical language. We provided calculators for those that wanted them. While the instructions pointed out that the interest rate might change, we did not indicate if, when or by which amount a change would happen. Note also that students could neither trade on margin, nor short sell assets.

A translation of the experimental instructions is in the appendix. The appendix also has screen shots of the experiment.

2.2 The experimental policy

Our interest rate policy algorithm aims to approximate the behavior of central banks such as the Federal Reserve Board (Fed). The basic idea is to raise rates when we see a positive bubble, and to lower them in the opposite situation (where 'bubble' is defined as persistent trade at values significantly (more than 50 percent) different from the NPV of our stock).

We do not change the interest rate more frequently than every 4 periods,

and not before period 4, for three reasons: one, to not introduce extra noise through continual adjustments; two, to keep participants from guessing when the next change would happen; and three, to approximate another 'real' Fed policy - frequent changes are unusual because the market might perceive them as informative (usually negative) signals in themselves.⁴

After interventions in one direction, central banks will attempt to create some leeway for further interventions during times that are less problematic - e.g., by lowering interest rates to an intermediate level when markets cool off after raising them first. Because, for statistical reasons, we restricted ourselves to only five different interest rates (0.01, 0.05, 0.11, 0.15, 0.21), we actually faced the same problem. Therefore, after interventions in one direction, we intervened instantly into the opposite direction when mean contract prices hit the stock's NPV (which happened only once).

We only used an endogenous algorithm for experiments with inexperienced subjects. In order to make statistical comparisons meaningful, we subjected groups of experienced traders to the same interest rates they had witnessed before.

The appendix contains the code of the interest rate algorithm.

2.3 Procedures

2.3.1 Procedures common to all experiments

Everyone initially received the same endowment in cash and stocks, but we did not tell the participants. There is sufficient evidence (see e.g. King, Smith, Williams, van Boening (1993), Porter, Smith (1995) or Caginalp, Porter, Smith (1998)) that initial heterogeneity of cash or stock accounts does not significantly influence the results of bubble experiments. Because the same is not true for *total* (consolidated) endowments, we controlled for the latter by providing the same initial endowments in all experiments (10 shares of stock and 3,600 Gulden per participant⁵).

All subjects were undergraduates from the University of Zurich and the Eidgenoessische Technische Hochschule/Zurich (ETH). The IEW maintains a large database of about 3,000 - 4,000 students who they recruit at the beginning of the academic year to participate in "economic experiments in decision making." Experimental subjects are called upon for participation when needed, their participation and success recorded for future reference, and generally paid a SFR 10 (\$ 6) showup fee in addition to a success dependent bonus at the end of experiments.

All prior bubble experiments establish the influence of experience on the trading behaviour of participants. We thus included a number of sessions with once-experienced traders. We divided each session into two groups of 7 or 8

⁴In this respect, 2001 is a very unusual year.

⁵The numbers are from a recent experiment by Lei, Noussair, Plott (2001). More precisely, they provide either 7,200 Gulden and 0 shares, or 0 Gulden and 20 shares of stock per participant. While this was in line with their research focus (bubbles without the possibility of speculative gains), we had to adjust it to our setting.

participants who received the same dividends. One group served as the treatment (policy) group, the other as the control (no policy) group. Experiments with inexperienced participants lasted on average 2 hours and 30 minutes, those with experienced participants about 1 hour and 15 minutes. Table 1 gives an overview over all session.

After the experiment, the participants exchanged their Gulden into Swiss Franks at a rate known to them from the start. Although we calibrated the experiments so that the average participant received a compensation comparable (by hour) to a Swiss student salary, we also created incentives for the participants to trade to the best of their abilities. Final payoffs ranged from roughly SFR 13.00 (including a showup fee of SFR 10.00) to SFR 80.00 (about \$ 7 - \$ 50) - a sizeable success dependent spread.

The trade software was **Z-Tree**. **Z-Tree** is a modular C++-based language originally conceived by Urs Fischbacher (1999) for economic experiments at the Institut fuer Empirische Wirtschaftsforschung of the University of Zurich⁶. In short, experimental participants interact with each other with the help of client terminals and are supervised by a server (the form of a local area network (LAN)), a structure that allows for the fast interventions necessary for our experiments.

2.3.2 Generic experiments

As it turned out, most experiments had the same interest rates. We call them *generic experiments* (table 2).

As a shortcut, we used the labels:

1. Generic experiments with inexperienced traders:

- (I,P): Policy
- (I,N): No policy

2. Generic experiments with experienced traders:

- (E,P): Policy
- (E,N): No policy

Figure 2 shows the NPV of a generic (I,P) experiment. We assume static expectations, i.e. that participants expect the current interest rate to persist until the end of the experiment. Naturally, this will not be true for every participant in every experiment. Still, given the structure of our experimental policy (which bounds the number of interventions from above by 3), and given that the experiment is short, we see no reason to believe that participants systematically deviate from this assumption. We adopt it as a working hypothesis, and make no further mention of it.

⁶Z-tree can be downloaded in exchange for a free licence at <http://www.iew.unizh.ch/ztree/howtoget.php>.

Session	Policy	Experienced	Subjects	Date conducted
1a	Yes	No	8	11/29/2000
1b	No	No	8	11/29/2000
2a	Yes	No	8	12/06/2000
2b	No	No	8	12/06/2000
3a	Yes	No	8	12/14/2000
3b	No	No	8	12/14/2000
4	Yes	Yes	8	12/14/2000
5a	Yes	No	8	12/15/2000
5b	No	No	8	12/15/2000
6a	Yes	No	8	01/25/2001
6b	No	No	8	01/25/2001
7	No	Yes	8	01/25/2001
8a	Yes, non-generic	No	8	01/26/2001
8b	Yes, non-generic	No	8	01/26/2001
9a	Yes	No	8	01/29/2001
9b	No	No	8	01/29/2001
10	Yes	Yes	8	01/29/2001
11a	Yes	No	7	02/01/2001
11b	No	No	7	02/01/2001
12	Yes	Yes	7	02/01/2001

Sessions a and b were held simultaneously with the same dividend draw for each group

Policies were generic where not indicated differently

Experiment 1 suffered from a software glitch and is not used for statistical tests

Table 1: Basic summary statistics of the experimental sessions

Generic experiments				
Treatment	Period			
	1-3	4-7	8-11	12-15
Policy	0.05	0.11	0.15	0.21
No policy	0.05	0.05	0.05	0.05
Non-generic experiments				
Period	Non-generic I			
	1-4	5-7	8-11	12-15
Interest rate	0.05	0.11	0.05	0.05
Period	Non-generic II			
	1-5	6-9	8-11	12-15
Interest rate	0.05	0.01	0.11	0.15

Table 2: Generic and non-generic experiments

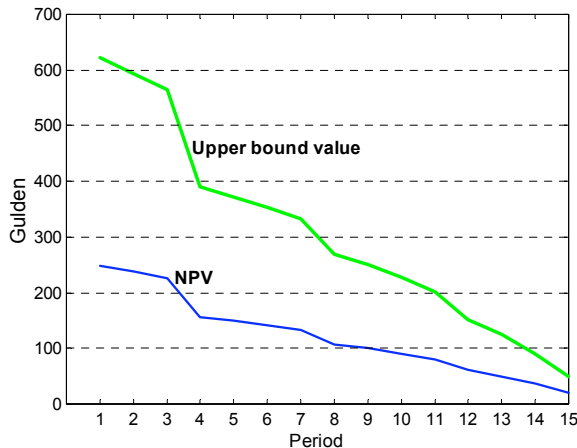


Figure 2: NPV of generic (I,P) experiment

2.3.3 Non-generic experiments

Additionally, we conducted two experiments with non-standard policies. We did not include them into any statistical test, but they may serve for future research (see table 2).

3 Analysis

We first define different measurement variables such as size and duration of bubbles, intensity of trade, portfolio choice, market volatility and so on. We then formulate some research hypotheses that we are able to investigate with the help of our experiment. In order to be able to check our conjectures, we quantify our measurement variables by treatment. We propose a fairly standard model of noise trading. A detailed analysis of our research hypotheses concludes this section.

3.1 Bubble measures and treatment variables

Much of what follows focuses on the following variables:

1. *Deviation*: $Deviation(s)$ is the standardized distance of observed mean contract prices from prices that would ensue if risk-neutral traders believing in dividend-discount models were trading under common knowledge in a stock with the described dividend structure and lifetime; $deviation(n)$

the distance to the expected *undiscounted* dividend value. Formally,

$$\begin{aligned} \frac{1}{15} \sum_{t=1}^{15} \frac{|P_t - f_t|}{80} & \quad deviation(s) \\ \frac{1}{15} \sum_{t=1}^{15} \frac{|P_t - Div_t|}{80} & \quad deviation(n). \end{aligned}$$

P_t and f_t are mean contract price respectively net present value (NPV) of the stock in period t , and

$$Div_t = \sum_{i=t}^{15} E[div_i], \quad t \in \{1, 2, \dots, 15\}.$$

$E[div_t]$ is the expected dividend in t , i.e. in this experiment $E[div_t] \equiv 24$. We normalize deviation by the number of stocks outstanding (80), which is constant because we do not retire stock before the end of the experiment. Intuitively, we thus obtain a measure akin to overvaluation per share. We also normalize to make the bubble measures comparable in size with each other.

2. *Relative bubble* measures the size of the bubble relative to fundamental value. Its formal definition is as

$$\frac{1}{15} \sum_{t=1}^{15} \frac{|P_t - f_t|}{f_t}.$$

3. *Duration*: Maximum number of consecutive periods during which the mean contract price increases relative to the net present value (NPV) of the stock, i.e. formally

$$\max_{1 \leq t \leq 15} \{m : P_t - f_t \leq P_{t+1} - f_{t+1} \leq \dots \leq P_{t+m} - f_{t+m}\},$$

where P_t and f_t are as before.

4. *Amplitude*: A measure of the overall size of the bubble - the normalized difference of the largest and smallest deviation of mean contract prices from the net present value (NPV) of the stock. We normalize by the net present value of period 1. Formally, *amplitude* is defined as

$$\max_{1 \leq t \leq 15} \left\{ \frac{P_t - f_t}{f_1} \right\} - \min_{1 \leq t \leq 15} \left\{ \frac{P_t - f_t}{f_1} \right\}$$

5. *Volatility*: A measure of the overall volatility of trade prices in all periods. To make this number meaningful in comparison, we normalize again. We first calculate the normalized volatility of trade prices for each period, i.e.

$$V_t = \frac{\sqrt{Variance(\text{trade prices in period } t)}}{P_t}, \quad t \in \{1, 2, \dots, 15\},$$

which is then averaged out over all periods:

$$\frac{1}{15} \sum_{t=1}^{15} V_t.$$

Volatility is an indicator of the market's overall volatility, not a volatility itself by any standard definition.

6. *Turnover*: The total volume of trade over all 15 periods divided by the number of shares outstanding, *turnover* is an indicator of trade intensity in the experiment.

3.2 Research Hypotheses

We formulate five conjectures and try to shed some light on them with our experiments. Some of the conjectures examine hypotheses from prior research in our setting (in particular Caginalp, Porter, Smith (1998), Smith, van Boening, Wellford (2000), and Lei, Noussair, Plott (2001)). Others investigate questions that are specific to our research design. Several have potential implications for economic policy. We generally use the intuitive abbreviation B(I,P) to indicate the extent of a bubble in an experiment with inexperienced traders and interest policy, and similar abbreviations for the other cases; this should be understood as a semantic variable. At the end of each conjecture, we indicate whether we were able to uphold or refute it based on our analysis of section 3.5. If the evidence is ambiguous, we instead conclude that the conjecture is "mostly upheld (refuted)", or "inconclusive."

Let's first check for bubbles:

Conjecture 1 *Basic bubble hypothesis (backward induction hypothesis)*. *A common definition has a bubble as sustained price deviation from fundamental value. If traders interact with each other under common knowledge of the market structure and backward induct correctly, such bubbles would not occur (see, e.g., Tirole (1982)). Therefore, consider first the conjecture that bubbles unanimously do not happen across all treatments, i.e. assume that $B(I,P)=B(I,N)=B(E,P)=0$. (refuted)*

A higher interest rate impacts value in two ways. Since bonds and stocks are gross-substitutes, an increase in interest rates should decrease stock prices and bubble sizes - at least in the case of naive bubbles. On the other hand, a higher interest rate creates additional income. There is some evidence that additional income increases the magnitude of a bubble (both Caginalp, Porter, Smith (1998) and Smith, van Boening, Wellford (2000) conjecture this, but only the former provide statistically significant support). Because of the presence of portfolio alternatives in our experiments, it is unclear whether greater wealth will translate into higher trade liquidity: while relative investment in bonds should be higher in (I,P) experiments, it is less clear what it means for absolute

trade liquidity. Overall, however, experiments with interest policy should tend to reduce bubbles.

Conjecture 2 *Policy effect on bubbles and market liquidity.* *The interest policy raises opportunity costs; although it increases total wealth, it decreases trade liquidity. Both factors tend to reduce the intensity and likelihood of the occurrence of bubbles, i.e. $B(I,P) < B(I,N)$. (mostly upheld)*

A recent paper by Lei, Noussair, Plott (2001) has experimental subjects trade an asset, and simultaneously in a market for what can be considered a service. It turns out that such a design reduces errors in decision making compared to a benchmark of stock trading only (Typical errors in decision making - or more precisely, non-theory conform trading - would be trades at more than the upper bound value (UBV)). In Lei et al's setting, the value of a stock is completely uncorrelated to the service market: the only feedback is that time spent in one market is time less spent in another. In our case, correlation is high: directly, by how interest rates change the NPV, but also indirectly, through the endogeneity of the interest rate policy algorithm. It is our conjecture that the earlier results were partly due to the fact that their experiment had completely segregated markets.

Conjecture 3 *Uncertainty hypothesis.* *The interest rate policy increases the uncertainty in the stock market: price volatility grows; trading patterns explicable by lack of common knowledge of rationality are less likely, while those pointing to actual irrationality are more likely. (upheld)*

What about portfolio choice more general?

Conjecture 4 *Value driven investment hypothesis (portfolio choice).* *Experimental participants recognize investment opportunities (their portfolio choice reflects them). Walrasian price adjustment accounts for differences in profitability - if these differences are unanticipated⁷. As a result, the presence of investment opportunities increases the success dependent spread in income. (upheld)*

Conjecture 3 relates to what has been called the active participation hypothesis (APH) by Lei, Noussair, Plott (2001). In their own words, the APH says that *a fraction of the volume in the markets is related to the fact that participation in the asset market is the only activity available for subjects*. If market participation were indeed solely due to the lack of available alternative activities, markets should not systematically differ with respect to errors in decision

⁷Investment professionals know well about the importance of surprises. Before meetings of the Board of Governors, market pundits provide consensus estimates of the expected decision of the Fed. If expectations are merely met, a change in interest rates has a modest to no impact on the stock market. To merely confirm what has been anticipated before may even be counterproductive. As an example, this is what happened on 03/20/2001 when the Fed lowered interest rates by 50 base points as had been generally expected. The result was that markets tumbled - the Dow Jones Industrial Index almost ended the day in bear territory for the first time in 10 years. The situation was particularly grave because about a third of the forecasts had predicted a change as dramatic as of 75 base points.

making. Conjecture 4 points in the same direction: actions and choices are deliberate and directional. We thus have

Conjecture 5 *Irrelevance of the active participation hypothesis.* *Our experiment does not support the active participation hypothesis (APH). Other factors seem to influence the trading behavior more, such as the recognition of value. (upheld)*

3.3 Overview

Figure 3 shows bubbles across different treatments, from - as we call them - a naive, a sophisticated, and a relative perspective (see section 3.1 for definitions). The numbers are averages over all experiments by treatment, but they are representative for the individual experiments as well (the appendix contains graphs for all experiments). We normalized the bubble measures so they correspond to overvaluation per share. Figure 3 also charts the mean contract volume (or turnover) per period per treatment, and the volatility of the turnover.

Note first that any differences for periods 1 to 3 are likely due to the small sample nature of our experiments (6 sessions per treatment in the inexperienced case), because the first policy intervention was after period 3. This said, figure 3 shows that the only clear treatment effect is that of experience. The interest policy also slightly decreases the size and duration of bubbles in inexperienced sessions (most notably naive bubbles after period 4). Note also that the size of relative bubbles tends to increase over time in no policy session, whereas the interest policy reverses this tendency after an initial increase. Briefly, the bubble sizes compare as in $\mathbf{B(I,N)} \geq \mathbf{B(I,P)} > \mathbf{B(E,P)}$ ⁸. The same is true for turnover.

Another point has to our knowledge not been noticed before. Both the graphs of naive and of sophisticated bubbles converge to 0 over time. Earlier authors have generally argued that this reconciles bubble with the rational expectations hypothesis (because, as they point out, it indicates Bayesian learning). But recall that we are in a situation in which one variable, f_t , converges to 0, as well as another, bub_t (representing either of the first two bubble measures). How much do we learn then from the fact that $bub_t \rightarrow 0$? This could still mean that $\frac{bub_t}{f_t} \uparrow +\infty$ although f_t and bub_t both go to zero - but at different orders of magnitude. The relative bubble chart shows that, approximately, $\frac{bub_t}{f_t} \rightarrow 1.5$ in both inexperienced treatments. The experienced sessions approach a value of about 1. While we do not believe that the value of 1.5 (or 1) has any particular significance, we should still keep this in mind when talking about bubbles that "...converge to zero" (Smith, van Boening, Wellford (2000)).

Figure 4 shows the average trade volume in more detail - turnover is very comparable in size across treatments in inexperienced sessions -, figure 5 the volatility of mean contract prices by treatment.

⁸We exclude the (E,N) treatment to keep the graphs more readable. We are not interested in (E,N) experiments per se and used them only as a benchmark for the baseline experiments. They are also included in later statistical tests.

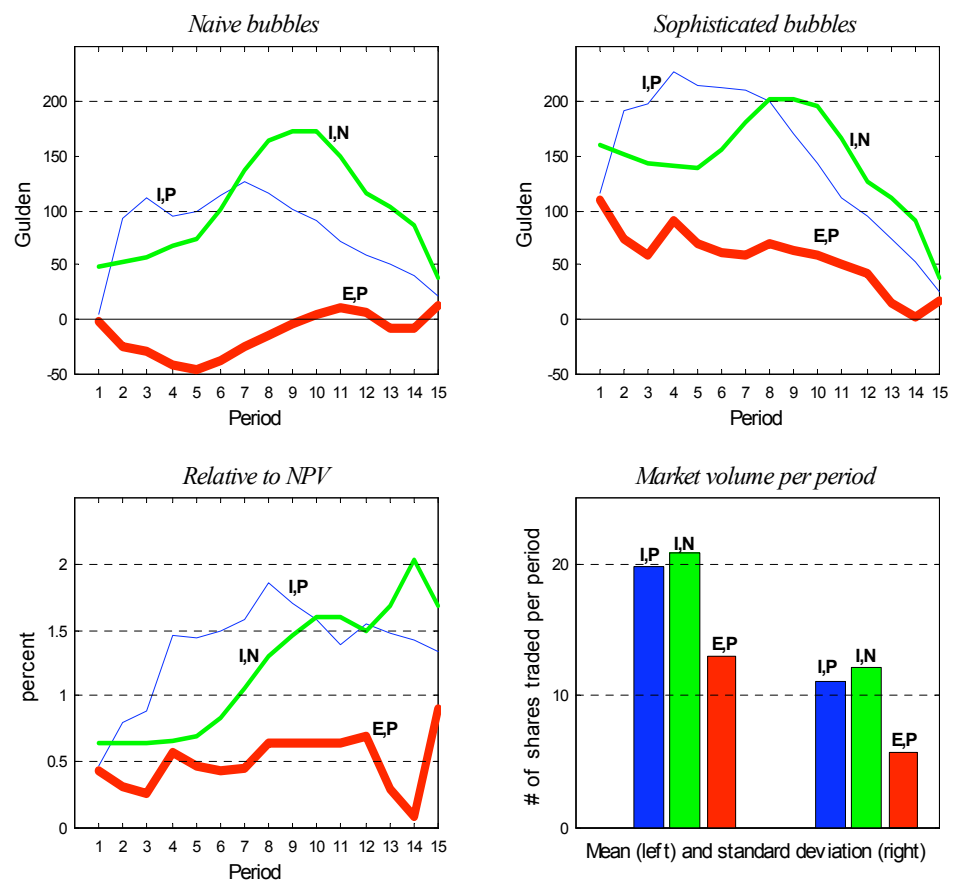


Figure 3: Bubbles and trade volume in experimental stock markets by treatment

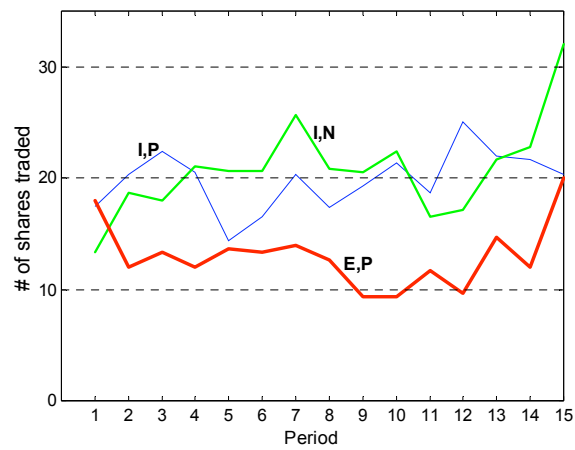


Figure 4: Trade volume in stock markets

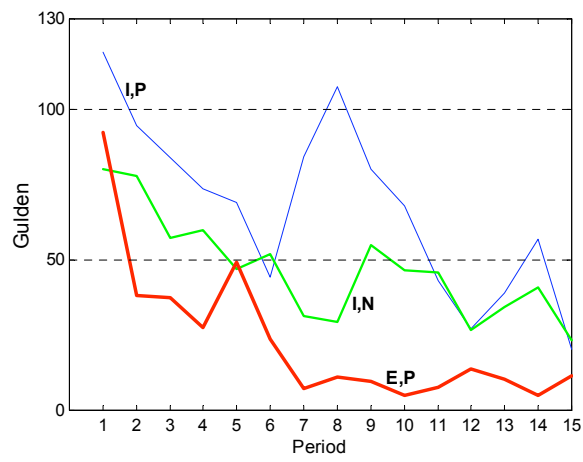


Figure 5: Volatility of mean contract prices

Measure	Treatment			
	I,P	I,N	E,P	E,N
Duration	3.33	5.83	3	2
Turnover	3.77	3.96	2.55	2.83
Amplitude	1.02	1.08	0.65	0.36
Price variance	0.28	0.18	0.13	0.10
Norm. deviation (naive)	1.14	1.64	0.48	0.15
Norm. deviation (soph.)	1.91	2.02	0.80	0.58
Relative bubble	1.36	1.20	0.50	0.31

Table 3: Bubble measures by treatment

The variance of mean contract prices exhibits a clear treatment effect. In experiments with interest policy, it is in all but one period an upper envelope of the volatility in control experiments with no interest policy. Notice the considerable spike in one of the 3 intervention periods (period 8), which in our eyes reflects the uncertainty introduced to the system through the interest policy. If we admit price variance as a bubble measure, the volatility chart implies a ranking of $\mathbf{B(I,P)} > \mathbf{B(I,N)} > \mathbf{B(E,P)}$.

3.4 Quantification of bubbles and simple explanatory models

3.4.1 Measurement of bubbles

Table 3 shows the values of the bubble measures for all treatments.

We usually conducted parametric and non-parametric tests of our hypotheses because we often had to deal with small samples. If both tests point to the same result, we accept it; if one test is significant but not the other, we try to find further evidence. Table 4 shows the test results. Both two-sample t-tests and Wilcoxon tests check for differences in the distribution (location) of two random vectors. They are essentially equivalent. The Wilcoxon test (also known as Ranksum, or Mann-Whitney test) is non-parametric and distribution free, and hence usually fares better if the underlying distribution is non-normal, or the sample size is small.

Measure	Null Hypothesis	t-statistic	p-value	Wilcoxon	p-value
Duration	H0: I,P < I,N	-1.239	(0.122)	-0.973	(0.165)
	H0: I,P > E,P	0.210	(0.420)	0.523	(0.301)
Amplitude	H0: I,P < I,N	-0.458	(0.328)	<10 ⁻⁴	(1.000)
	H0: I,P > E,P	2.243	(0.030)	1.549	(0.061)
Price variance	H0: I,P > I,N	1.748	(0.056)	1.761	(0.039)
	H0: I,P > E,P	2.052	(0.040)	2,324	(0.010)
Deviation(s)	H0: I,P < I,N	-0.252	(0.403)	-0.480	(0.316)
	H0: I,P > E,P	2.810	(0.013)	2.324	(0.010)
Turnover	H0: I,P = I,N	-0.183	(0.857)	-0.480	(0.631)
	H0: I,P = E,P	1.133	(0.294)	1.033	(0.302)
Rel. bubble	H0: I,P < I,N	-0.227	(0.587)	0.480	(0.316)
	H0: I,P > E,P	1.924	(0.048)	2.324	(0.020)
Deviation(n)	H0: I,P < I,N	-1.368	(0.101)	-1.601	(0.055)
	H0: I,P > E,P	1.778	(0.059)	1.807	(0.035)

Table 4: Test of differences in bubble measures

$$\begin{pmatrix} d_{P2} \\ d_{P3} \\ d_{P5} \\ d_{P6} \\ d_{P9} \\ d_{P11} \\ d_{N2} \\ d_{N3} \\ d_{N5} \\ d_{N6} \\ d_{N9} \\ d_{N11} \\ d_{EP4} \\ d_{EP10} \\ d_{EP12} \\ d_{EN7} \end{pmatrix} = \begin{pmatrix} 1 & 0 & 0 \\ 1 & 0 & 0 \\ 1 & 0 & 0 \\ 1 & 0 & 0 \\ 1 & 0 & 0 \\ 1 & 0 & 0 \\ 0 & 1 & 0 \\ 0 & 1 & 0 \\ 0 & 1 & 0 \\ 0 & 1 & 0 \\ 0 & 1 & 0 \\ 0 & 1 & 0 \\ 1 & 0 & 1 \\ 1 & 0 & 1 \\ 1 & 0 & 1 \\ 0 & 1 & 1 \end{pmatrix} \begin{pmatrix} \beta_{d_P} \\ \beta_{d_N} \\ \beta_{d_{Exp}} \end{pmatrix} + \begin{pmatrix} \varepsilon_{d1} \\ \varepsilon_{d2} \\ \varepsilon_{d3} \\ \varepsilon_{d4} \\ \varepsilon_{d5} \\ \varepsilon_{d6} \\ \varepsilon_{d7} \\ \varepsilon_{d8} \\ \varepsilon_{d9} \\ \varepsilon_{d10} \\ \varepsilon_{d11} \\ \varepsilon_{d12} \\ \varepsilon_{d13} \\ \varepsilon_{d14} \\ \varepsilon_{d15} \\ \varepsilon_{d16} \end{pmatrix} \quad (1)$$

$$\Longleftrightarrow: d = M\beta_d + \varepsilon_d$$

We also conducted a regression analysis of the bubble measures using seemingly unrelated regression (SUR). In the parametric case, we had six sets of regression equations. A generic equation is of the form of (1). ε_d is white noise, d_{ij} is the value of the variable duration in treatment i (P=(I,P), N=(I,N), EP=(E,P), EN=(E,N)) and experiment j (see table 1). The coefficient β_{d_P} is the baseline (inexperienced) coefficient for *generic* experiments with interest policy. β_{d_N} is the baseline (inexperienced) coefficient for *generic* experiments with interest rate fixed at 5 %. $\beta_{d_{Exp}}$ measures the impact if the experimental

subjects were once experienced, i.e. had already participated in a prior run of the same experiment.

The other bubble measures are t (turnover), a (amplitude), v (volatility), nd (deviation - sophisticated or naive) and rb (relative bubble). Clearly, there are possible cross-equation correlations, so we stack the different equations as in (2) and perform a SUR estimation.

$$\begin{pmatrix} d \\ t \\ a \\ v \\ nd \\ rb \end{pmatrix} = \begin{pmatrix} M & 0 & 0 & 0 & 0 & 0 \\ 0 & M & 0 & 0 & 0 & 0 \\ 0 & 0 & M & 0 & 0 & 0 \\ 0 & 0 & 0 & M & 0 & 0 \\ 0 & 0 & 0 & 0 & M & 0 \\ 0 & 0 & 0 & 0 & 0 & M \end{pmatrix} \begin{pmatrix} \beta_d \\ \beta_t \\ \beta_a \\ \beta_v \\ \beta_{nd} \\ \beta_{rb} \end{pmatrix} + \begin{pmatrix} \varepsilon_d \\ \varepsilon_t \\ \varepsilon_a \\ \varepsilon_v \\ \varepsilon_{nd} \\ \varepsilon_{rb} \end{pmatrix} \quad (2)$$

$$\iff : y = X\beta + \varepsilon$$

It has been recommended elsewhere (Conover (1999)) that in experimental designs for which no non-parametric tests exist one should use the usual analysis of variance on the data and then perform the same procedure on the rank transformed data. We thus ranked our experimental data in ascending order and ran another SUR on the ranked data. The results of both the parametric estimation and the estimation using rank-transformed data are in table 5.

Table 6 contains the results of some hypothesis tests we performed on the data from table 5. The statistics shown are t- respectively F-tests; in the case of the non-parametric regressions, these tests are equivalent to Mann-Whitney respectively Kruskal Wallis tests (Conover (1999)). Both series of tests generally point in the same direction (on the 10 % significance level they agree in every case).

The coefficients of table 3 indicate that in experiments with inexperienced traders the bubble measures - with the exception of price variance - are smaller in experiments with interest policy than in baseline no policy experiments. In other words, except for volatility, interest policy has a positive - if small - impact on bubbles. We next checked whether the evidence is statistically significant (table 4). The data only modestly support that bubble measures are smaller in policy treatments (on a 10 % significance level, this holds only for naive bubbles, with a more generous decision criterion also for duration). Price variance clearly increases in policy experiments.

The regressions give a more insightful feedback on these comparisons because they show cause and effect. The coefficients are generally highly significant (table 5), except for experienced coefficients for turnover and duration (the coefficient for turnover is slightly significant). Table 6 contains the results of statistical tests. The test of equality of the bubble measures ($\beta_P = \beta_N$) cannot be rejected, except for volatility again. The impact of experience is generally positive ($\beta_{exp} \geq 0$), except for duration. All tests strongly reject the more extreme hypothesis that $\beta_P = \beta_N = 0$. Summing up, and excluding volatility for the moment, the data are compatible with a relative ranking of

Parametric estimation							
Coefficient	Dependent variable						
	Dura- tion	Ampli- tude	Price variance	Devi- ation(s)	Turn- over	Rel. bubble	Devia- tion(n)
β_p	3.68	1.06	0.23	1.23	3.77	1.38	1.95
(p-val.)	(0.001)	(<10 ⁻⁴)	(<10 ⁻⁴)	(<10 ⁻⁴)	(<10 ⁻⁴)	(<10 ⁻⁴)	(<10 ⁻⁴)
β_N	5.48	1.05	0.14	1.56	3.97	1.26	1.99
(p-val.)	(<10 ⁻⁴)	(<10 ⁻⁴)	(<10 ⁻⁴)	(<10 ⁻⁴)	(<10 ⁻⁴)	(<10 ⁻⁴)	(<10 ⁻⁴)
β_E	-1.38	-0.48	-0.10	-0.92	-1.20	-0.85	-1.22
(p-val.)	(0.435)	(<10 ⁻⁴)	(0.027)	(0.004)	(0.149)	(0.006)	(0.001)
R^2	0.67	0.96	0.84	0.84	0.87	0.84	0.89
F-stat.	32.83	373.29	85.75	83.42	105.49	81.32	125.13
n	16	16	16	16	16	16	16

Ranked regressions							
Coefficient	Dependent variable						
	Dura- tion	Ampli- tude	Price variance	Devi- ation(s)	Turn- over	Rel. bubble	Devia- tion(n)
β_P	8.47	10.7	12.03	9.27	8.48	9.93	10.32
(p-val.)	(<10 ⁻⁴)	(<10 ⁻⁴)	(<10 ⁻⁴)	(<10 ⁻⁴)	(<10 ⁻⁴)	(<10 ⁻⁴)	(<10 ⁻⁴)
β_N	10.2	10.3	7.63	11.4	10.68	9.4	10.52
(p-val.)	(<10 ⁻⁴)	(<10 ⁻⁴)	(<10 ⁻⁴)	(<10 ⁻⁴)	(<10 ⁻⁴)	(<10 ⁻⁴)	(<10 ⁻⁴)
β_E	-2.9	-8.1	-6.43	-6.8	-3.78	-4.8	-7.62
(p-val.)	(0.244)	(<10 ⁻⁴)	(0.001)	(<10 ⁻⁴)	(0.116)	(0.050)	(<10 ⁻⁴)
R^2	0.81	0.90	0.88	0.89	0.82	0.82	0.89
F-stat.	67.90	146.39	115.47	132.12	74.15	71.49	130.38
n	16	16	16	16	16	16	16

Note: seemingly unrelated regression of measures 1-6 together. Measure 7 is from a separate SUR.

Table 5: Analysis of bubble measures

Parametric regressions							
	Dependent variable						
Null H.	Dura- tion	Ampli- tude	Price variance	Devi- ation(s)	Turn- over	Rel. bubble	Devi- ation(n)
$\beta_P = \beta_N$	1.35 (0.245)	0.01 (0.925)	4.14 (0.042)	1.35 (0.245)	0.08 (0.778)	0.19 (0.661)	0.01 (0.904)
$\beta_P = \beta_N$ $= 0$	29.46 ($<10^{-4}$)	339.61 ($<10^{-4}$)	77.60 ($<10^{-4}$)	81.27 ($<10^{-4}$)	91.51 ($<10^{-4}$)	77.61 ($<10^{-4}$)	114.48 ($<10^{-4}$)
$\beta_{Exp} \geq 0$	-0.781 (0.218)	-4.115 ($<10^{-4}$)	-2.215 (0.013)	-2.870 (0.002)	-0.144 (0.074)	-2.760 (0.003)	-3.298 ($<10^{-4}$)

Ranked regressions							
	Dependent variable						
Null H.	Dura- tion	Ampli- tude	Price variance	Devi- ation	Turn- over	Rel. bubble	Devi- ation(n)
$\beta_P = \beta_N$	0.64 (0.425)	0.07 (0.799)	6.38 (0.012)	1.69 (0.194)	1.09 (0.300)	0.75 (0.386)	0.01 (0.904)
$\beta_P = \beta_N$ $= 0$	59.76 ($<10^{-4}$)	143.67 ($<10^{-4}$)	108.35 ($<10^{-4}$)	128.55 ($<10^{-4}$)	67.51 ($<10^{-4}$)	134.03 ($<10^{-4}$)	127.42 ($<10^{-4}$)
$\beta_{Exp} \geq 0$	-1.166 (0.122)	-4.511 ($<10^{-4}$)	-3.224 (0.001)	-3.617 ($<10^{-4}$)	-1.570 (0.058)	-4.335 ($<10^{-4}$)	-4.027 ($<10^{-4}$)

First row are t- resp. F-statistics, the second p-values

Table 6: Hypothesis tests

$B(E,P) < B(I,P) \leq B(I,N)$, with a possible equality in the second comparison. Based on volatility, the ranking is unequivocally $B(I,P) > B(I,N) > B(E,P)$.

3.4.2 Measurement of liquidity, trading behavior and success

Liquidity Figure 6 charts consolidated total market liquidity, i.e. the sum of the total cash accounts of the participants by period; it also shows trade liquidity. As such, it provides evidence of the income effect (as opposed to the substitution effect) of interest policy. Remark the clear and cumulative impact of the higher interest rates in policy experiments - total liquidity increases markedly. On the other hand, experience does not influence total liquidity much.

Figure 7 charts the portfolio choice and absolute trade liquidity by treatment and compares the investment decision between treatments.

Absolute trade liquidity is higher in no policy treatments than in treatments with interest policy (conversely, absolute investment in bonds is higher in policy than in no policy experiments). Similarly, relative investment in bonds in (I,P) experiments is an upper envelope to relative investment in bonds in (I,N) experiments. Even more clearly, investment in bonds rises absolutely and relatively in experiments with experienced traders.

Figure 8 shows how this investment behavior varies across participants. In other words, it charts the heterogeneity of the share of the participants' portfolio invested in bonds. In every period, investment in bonds varies less in (I,P) experiments compared to (I,N) experiments.

Trading behavior We split the positive real line into three areas. Assume the highest possible dividend payment is drawn in each period and calculate the NPV of a stock with this certain dividend. As mentioned before, call this the *upper bound value* (UBV), and trades at more than UBV high (or 'h'). Trades at h are special because no attitude towards risk can justify value investment (as compared to investment for other reasons such as speculation, or computational errors) in the stock at a price of higher than UBV. Conversely, we call deals under the expected NPV of the stock low (or 'l'). Risk-neutral traders consider deals at l as a bargain. Finally, denote the interval between l and h by 'm' (or medium). Figure 9 charts these trading measures.

In (I,P) experiments, there are hardly any bargain deals, but a high number of expensive h trades. In (I,N) treatments, there are more bargains and less non-value driven h deals. Experience almost completely eliminates these non-value driven deals, and most trades are of moderate size.

Table 7 quantifies the informal comparisons of this subsection so far, and confirms them.

Total liquidity increases significantly in policy experiments, but not with experience. Both measures of trade liquidity (absolute and relative) show higher investment in bonds in policy experiments (statistically, the absolute increase is clearer). The focality of the increase, i.e. the reduction of volatility with respect to the share invested across participants, is strongly significant, whereas only experience reduces total trading volume. Finally, the number of trades

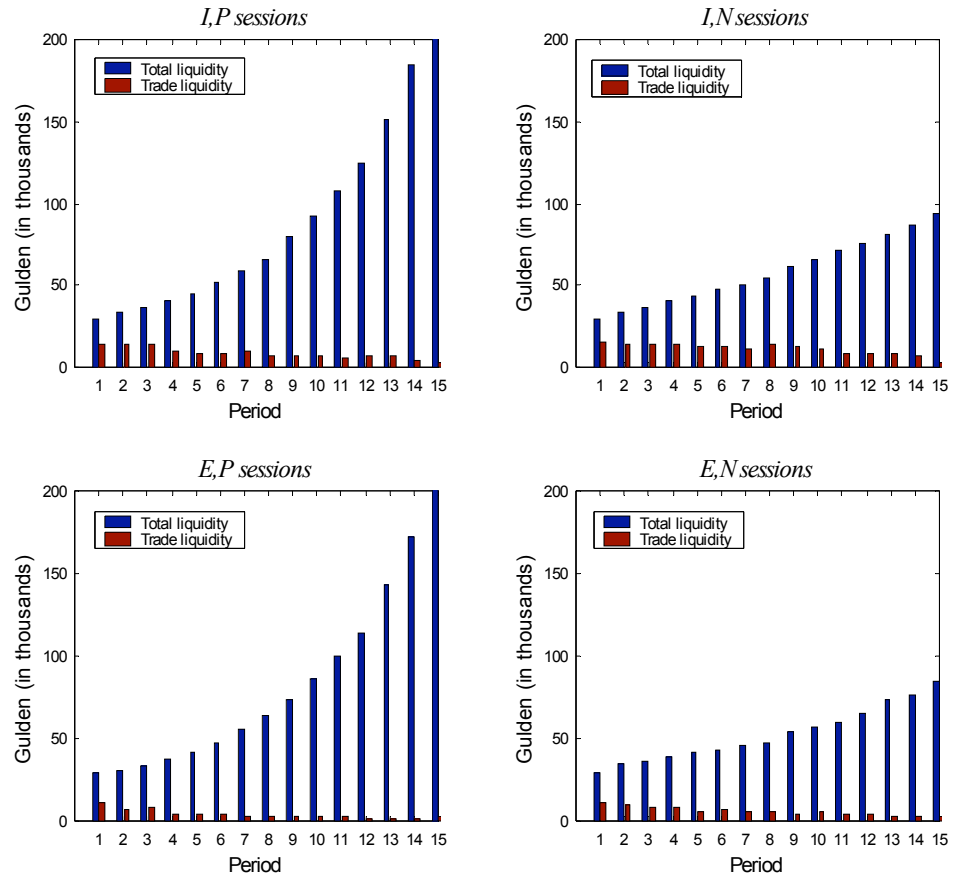


Figure 6: Total and trade liquidity

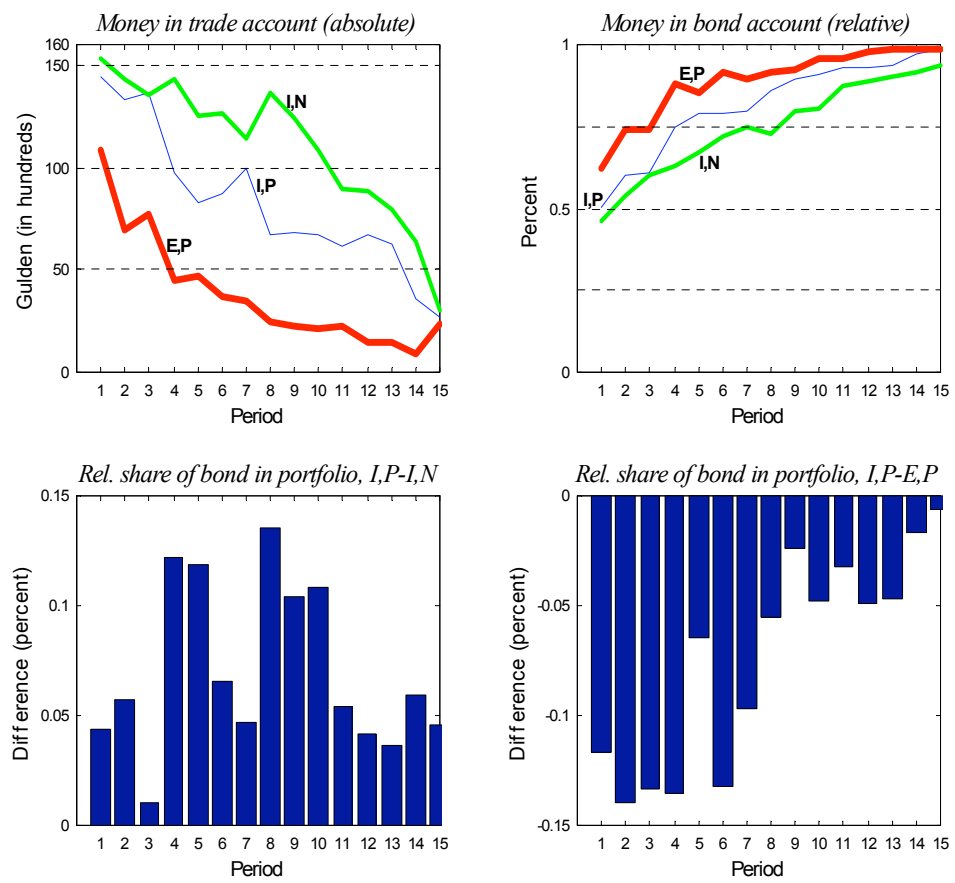


Figure 7: Money in trade account

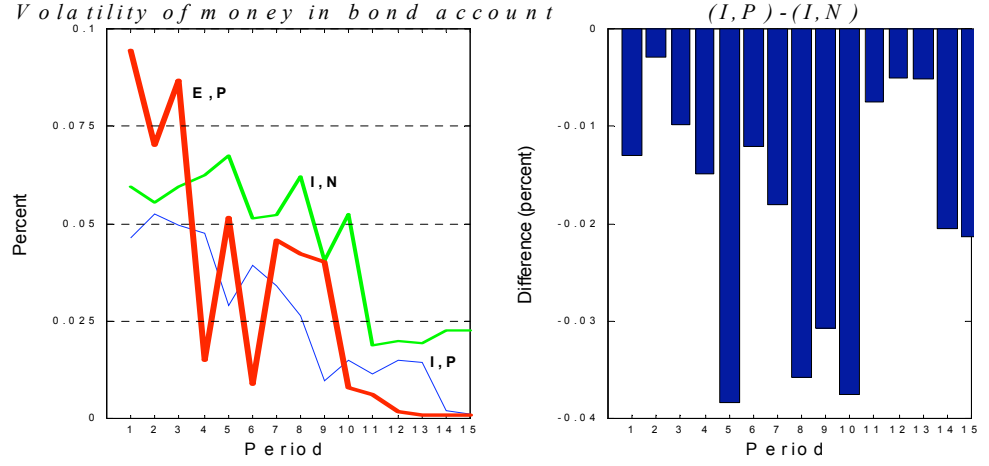


Figure 8: Volatility of money in bond account

Variable	Null Hypothesis	t-statistic	p-value	Wilcoxon	p-value
Total liquidity	H0: I,P > I,N	1.845	(0.037)	1.120	(0.131)
	H0: I,P = E,P	0.285	(0.778)	0.290	(0.772)
Trade liquidity	H0: I,P < I,N	-2.285	(0.017)	-2.053	(0.020)
	H0: I,P > E,P	3.883	(<10 ⁻⁴)	3.215	(0.001)
Relative investment in bonds	H0: I,P > I,N	1.308	(0.101)	1.431	(0.076)
	H0: I,P < E,P	-1.541	(0.067)	-1.431	(0.076)
Variance money in bonds	H0: I,P < I,N	-3.316	(0.001)	-3.626	(<10 ⁻⁴)
	H0: I,P < E,P	-0.772	(0.221)	0.630	(0.264)
Trading volume	H0: I,P = I,N	-0.555	(0.580)	-0.538	(0.591)
	H0: I,P > E,P	3.797	(<10 ⁻⁴)	3.418	(<10 ⁻⁴)
Trades at more than MDV	H0: I,P > I,N	1.056	(0.146)	1.799	(0.036)
	H0: I,P > E,P	5.166	(<10 ⁻⁴)	5.731	(<10 ⁻⁴)

Table 7: Testing differences in distribution

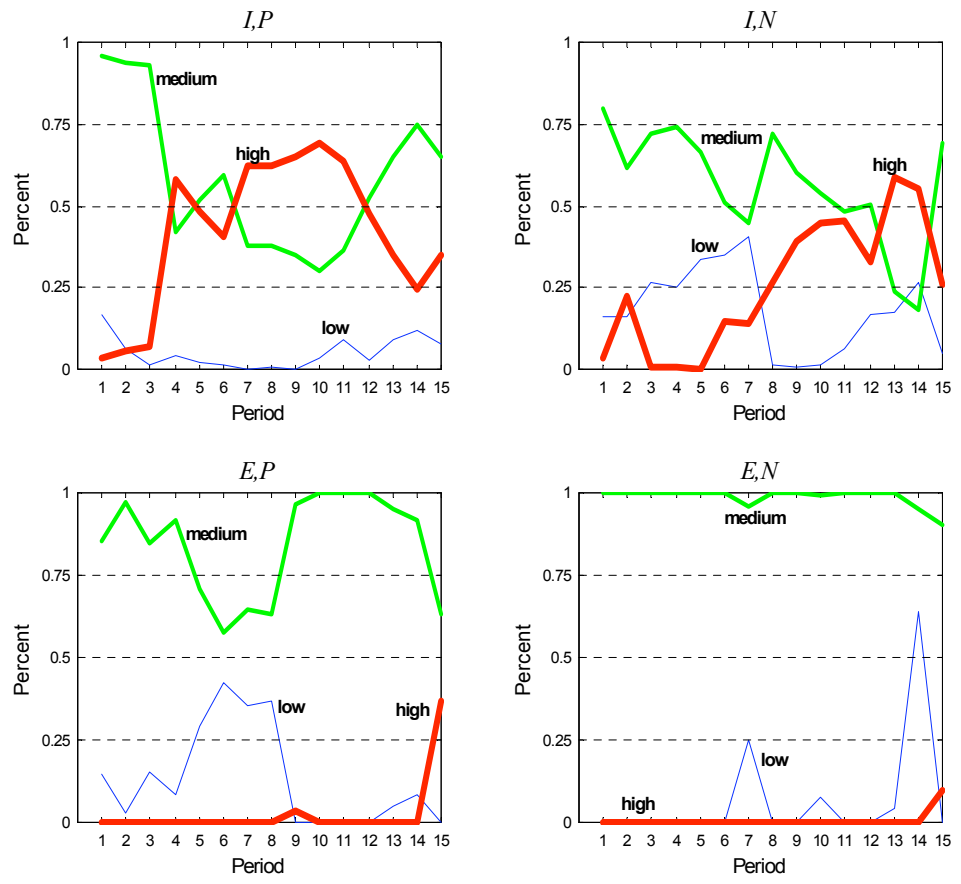


Figure 9: Level of non theory-conform trading

at more than MDV also significantly increases in (I,P) experiments over (I,N) experiments (clearer based on the Wilcoxon statistic). Both parametric and nonparametric tests agree that these trades go significantly down with experience.

Trading success⁹ All participants start the experiment with the same endowment of 10 stock and 3,600 Gulden. If they all did equally well, each participant would own $\frac{1}{8} \cdot (\text{total liquidity}_{15})$ at the end of the experiment. Figure 10 examines this question.

Clearly, this is not the case. Some people do better than expected, some worse. More interestingly, the success dependent spread in income distinctly differs in (I,P) and (I,N) markets: the best do better in (I,P) environments, and the worst fare worse. In other words, interest policy helps sort out good from bad traders. Experience accentuates this result: only two traders do better with experience, six do worse or similar to before, and the extent of over- and underperformance increases. We checked the statistical significance of the differences using Spearman's Rho¹⁰; both differences are highly significant.

3.4.3 A simple model of noise trading

The model We investigate whether we can capture some of the price dynamics in a simple, heuristic learning model. To this end, we develop a model of noise trading on a stock market. As is usual in the literature on this topic (see, e.g., Azariadas (1993) or Brock, Hommes (1998)), we assume that some of the traders are informed traders and base their evaluation of stock prices on fundamental values. Others are chartists (or momentum traders). We assume that chartists have adaptive expectations. This assumption has a long history in economics that goes as far back as, to our knowledge, Irving Fisher; it has also been found to correspond well to actual price forecasts of participants of prior laboratory stock market experiments (see Smith, Suchanek, Williams (1988)). Essentially, adaptive expectations describe chartists as trend-following. Informed traders expect that stock prices will, in the long run, approach fundamental values if they currently differ from them.

We now describe the model elements. A population of traders deals over 15 periods in a dividend-bearing stock on a stock exchange; t is time, indexing the periods, i.e. $t \in \{1, 2, 3, \dots, 15\}$. d_t is the dividend draw in period t . Dividends are finite-valued and have a finite, discrete distribution that is stationary over time. Let $d := \mathbf{E}_{t-1}[d_t] \equiv \mathbf{E}[d_t]$, with \mathbf{E}_{t-1} resp. \mathbf{E} denoting the conditional (unconditional) mathematical expectation, which coincide in our market struc-

⁹We are grateful to Robert Wilson who pointed out that it was important to investigate trading success.

¹⁰Spearman's Rho is what one obtains by replacing the observations by their ranks and then computing Spearman's product moment coefficient on the ranks. As usual with non-parametric statistics, its advantage over the Pearson coefficient is that Spearman's Rho is distribution free.

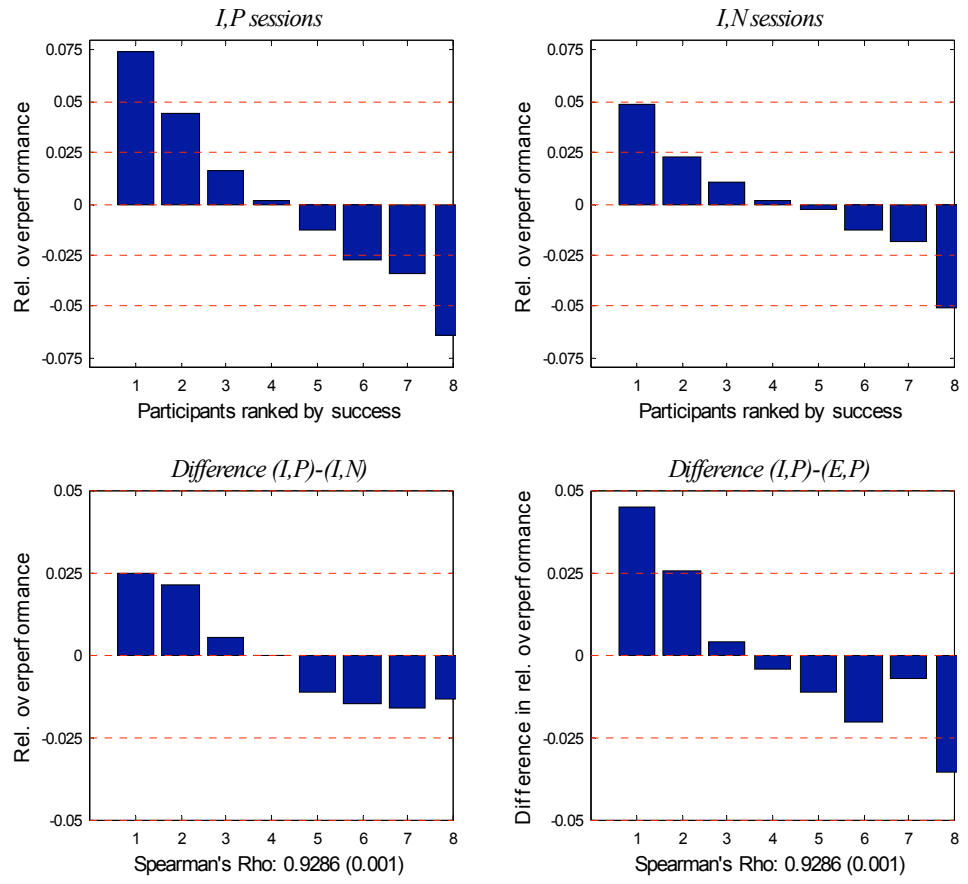


Figure 10: Success of participants relative to uniformly distributed income

ture.¹¹ d_t evolves over a trivial filtration of the form $\mathcal{F}_1 = \mathcal{F}_2 = \dots \mathcal{F}_{15} = \mathcal{F}$, \mathcal{F}_i being natural (minimal) for d_i .

We assume risk-neutral traders who have common knowledge of the market structure. In addition to the stock, there is a risk-free bond carrying an interest of r_t ¹². $X_t \in [0, 1]$ is the fraction of informed traders in the population of traders. We define two measures for the sensitivity of adjustment of informed and of noise traders' opinion, $\lambda_t^i, \lambda_t^n \in [0, 1]$; the significance of these two parameters will be clear in a moment. f_t and p_t are fundamental resp. actual stock prices in period t . The fundamental value is calculated from a standard risk-neutral dividend-discount model. Finally, $p_t^{e,i}$, $p_t^{e,n}$ and p_t^e are the price expectations for period t of informed traders, noise traders, and the market overall in period $t - 1$. From a behavioral point of view, these expectations are reference levels.

We now formalize the different expectations that we intuitively described above:

$$\begin{aligned} p_t^{e,i} &= p_{t-1} + \lambda_t^i (f_t - p_{t-1}) && \text{price expectation of informed traders} \\ p_t^{e,n} &= p_{t-1} + \lambda_t^n (p_{t-1} - p_{t-1}^e) && \text{price expectation of noise traders} \end{aligned}$$

For simplicity, we assume that the market expectation is just a weighted average of individual expectations of the participants. This leads to

$$p_t^e = X_t p_t^{e,i} + (1 - X_t) p_t^{e,n}$$

We assume the absence of arbitrage opportunities, i.e. that

$$p_t = \frac{d + p_{t+1}^{ex}}{1 + r_t},$$

where p_{t+1}^{ex} is some price expectation for period $t + 1$ in period t . It is a matter of simple algebra to calculate the equilibrium prices in the different market situations. In a market with noise traders and informed traders, i.e. under the expectation p_t^e , the equilibrium price is given by:

$$p_t^* = \frac{X_{t+1} \lambda_{t+1}^i f_{t+1} + (1 - X_{t+1}) (1 - \lambda_{t+1}^n) (1 + r_{t-1}) p_{t-1}^* + (1 - (1 - X_{t+1}) (1 - \lambda_{t+1}^n)) d}{1 + r_t - (1 - X_{t+1}) \lambda_{t+1}^n - X_{t+1} (1 - \lambda_{t+1}^i)},$$

assuming that the denominator is different from zero.

Fitting the model to the data It is not possible to fit the model uniquely to the experimental data because there are too many degrees of freedom. Hence, we make some simplifying assumptions. Under common knowledge of the market structure, one could claim that the sensitivity of adjustment - the parameters λ - depend only on the fraction of the respective traders in the population.

¹¹Formally, the conditional expectation is a random variable, which in our case is concentrated on one point, $\mathbf{E}[d_t]$.

¹²Remark the change in notation for the interest rate from i to r in order to keep it separate from the letter i that now indicates informed traders in our economy.

This amounts to saying that if 80 % of the traders are informed, the market opinion is that there is a move to fundamental values of $0.8 \cdot (f_t - p_{t-1})$, which is counterbalanced by a 20 % move based on the change of opinion of chartists, i.e. of $0.2 \cdot (p_{t-1} - p_{t-1}^{e,n})$. Formally, this means

$$\begin{aligned}\lambda_t^i &: = X_t \\ \lambda_t^n &: = 1 - X_t\end{aligned}$$

We can now fit our model to the experimental data. We are interested in checking which assumptions on the parameters of the model have to be made in order to produce the experimental outcome and to check whether the results follow a pattern that is intuitively appealing. One possible conjecture has the percentage of informed traders increase towards the end of the experiment, either because the participants learn to understand the model better, or simply because the for a 'rational' solution necessary backward induction gets easier over time. Learning and backward induction may result from conscious as well as unconscious thinking, along the lines of Simon's remark that one cannot "rule out the possibility that the unconscious is a better decision-maker than the conscious." (Simon (1955)).

Let $\vec{X} := (X_1, X_2, \dots, X_{15})^t$ be the vector of the proportion of fundamental traders by period. The theoretically market clearing price is then of the form $p^* := \vec{p}(\vec{X}) := (p_1(\vec{X}), p_2(\vec{X}), \dots, p_{15}(\vec{X}))^t$. Denote the observed mean contract price in experiment i by $\mathcal{P}^i = (\mathcal{P}_1^i, \mathcal{P}_2^i, \dots, \mathcal{P}_{15}^i)^t$. Stack prices as follows: $\mathcal{P} = (\mathcal{P}^1, \mathcal{P}^2, \dots, \mathcal{P}^n)^t$, where n is the number of experiments by treatment, and $p = \underbrace{(p^*, p^*, \dots, p^*)^t}_{n\text{-times}}$. Let $I := \prod_{i=1,2,\dots,15} [0, 1]$ be the 15-dimensional unit cube.

We estimate \vec{X} using non-linear least squares as¹³

$$\vec{X} = \arg \min_{\vec{X} \in I} (p - \mathcal{P})^t (p - \mathcal{P}).$$

Figures 11 and 12 sum up the results of our estimations.

The results of the no policy (I,N) und (E,N) sessions are roughly as conjectured from first principles. The proportion of trading due to 'rational' considerations increases over time, either because participants learn to understand the model better or because backward induction is easier close to the end of the experiment. Despite the negative spike in periods 9 and 10, we note the same basic pattern in (E,P) experiments. Only in (I,P) sessions this pattern is largely absent - remark that all intervention periods are local minima of the

¹³Non-linear least squares are the simplest form of estimation using the generalized method of moments (GMM). GMM proceeds by minimizing a quadratic form like $Q := (p - \mathcal{P})^t \Sigma (p - \mathcal{P})$ (Σ some positive definite weighting matrix); in the case of non-linear least squares $\Sigma = Id$ (the identity matrix). The form of the asymptotically optimal weighting matrix is well-known (see Hansen (1982) or also Andrews (1991)). Economic experiments usually work with small-samples, and so it is doubtful whether the use of an asymptotically optimal matrix would benefit the quality of estimation.

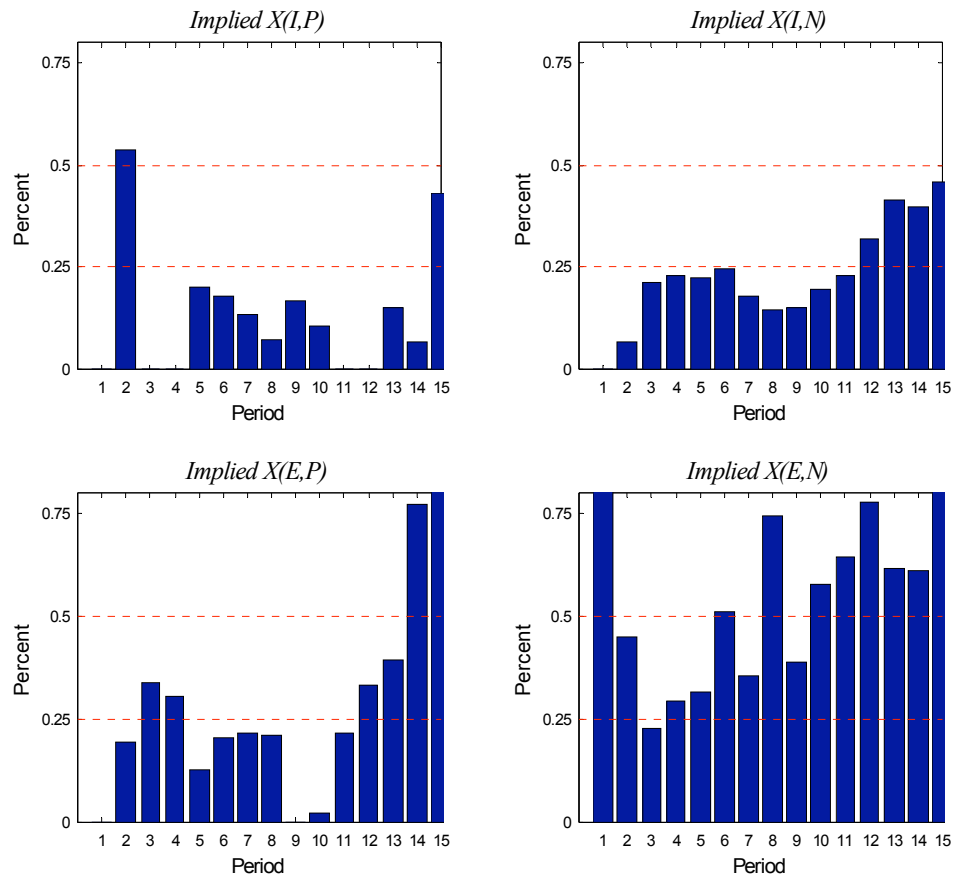


Figure 11: Implied proportion of 'rational' traders

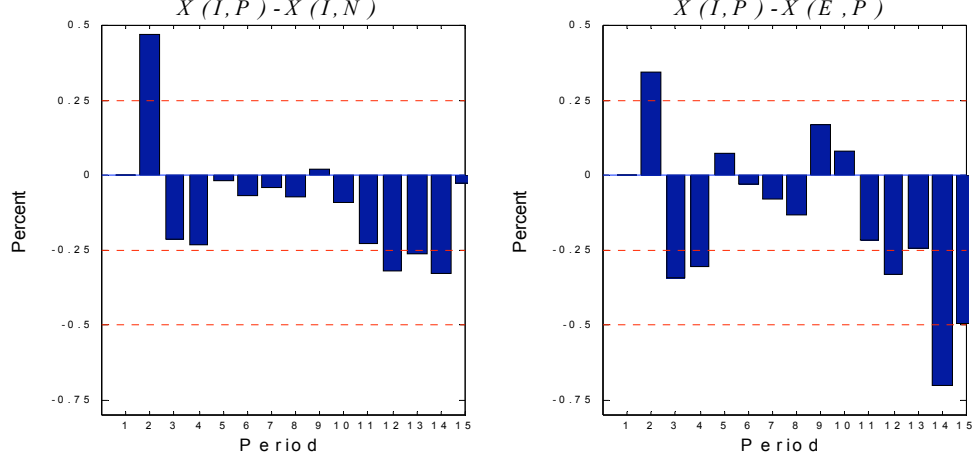


Figure 12: Comparison of implied proportion of 'rational' traders

implied share of rational trading. Something else is curious about the (I,P) treatments¹⁴. The graph of these sessions looks as if three learning cycles succeed each other. Each begins with minimal rationality in an intervention period (4, 8, and 12), after which the implied share of rational traders rises somewhat until the next intervention. Hence, (I,P) experiments look as if they consist of three small, disjoint versions of the results of the other treatments.

3.5 Analysis of the research hypotheses

Conjecture 1: C1 predicts that $B(I,P)=B(I,N)=B(E,P)=0$. Our data show that this is clearly not the case. There are bubbles based on every measure (see figure 3), and the bubble measures do not equal zero (tables 3 and 4). We reject the hypothesis that $\beta_P = \beta_N = 0$ on a highly significant level (table 6), and even more clearly the hypothesis that all coefficients are jointly zero (F-statistic in table 6). Hence, our data strongly refutes conjecture 1.

Conjecture 2: Based on figure 3, bubbles are much more pronounced in inexperienced treatments; the two inexperienced treatments differ less obviously from another. Tables 3 and 4 quantify this impression. Experience reduces all bubble measures except for turnover and duration. In addition, volatility, naive deviation and duration (on a 15 % level) significantly differ in (I,P) sessions from (I,N) sessions. Table 6 reports the results of several tests based on the regressions we conducted. Using this measure, we find only rarely statistically significant differences; namely, only the coefficient of price variance significantly differs in inexperienced sessions.

¹⁴Muhamet Yildiz pointed this out to us.

When we look at all the evidence for inexperienced sessions, we find that interest policy reduces the duration and deviation of bubbles compared to sessions with no interest policy; i.e., we have some support for our hypothesis, although it is not entirely clear. Experience, on the other hand, is found to have an unambiguous effect: almost all measures support $B(E,P) < B(I,P)$ and $B(E,N) < B(I,N)$.

It is immediately apparent from figure 6 and table 7 that higher interest rates create additional income (investing into bonds is more profitable). Despite of this, figure 7 shows that there is less money in people's trade accounts! (table 7 confirms that the differences are highly significant) We will discuss this surprising result below in conjecture 4; for now we just note that it conforms well with conjecture 2. It will also become clear later why the result is not so surprising after all.

Summing up, we have some evidence for conjecture 2.

Conjecture 3: The experiment clearly documents the increase in price volatility. All measures, from figure 3, table 3, table 4, to table 6, agree; in fact, this is the only difference that finds statistically significant support from the tests reported in table 6.

Figure 9 charts the trading behavior in the respective markets. We observe more trades at more than MDV in (I,P) than in (I,N) sessions (table 7 shows that the difference is significant, in particular based on the nonparametric Wilcoxon test).

As we have mentioned before, trades at h are difficult to reconcile with theory conform trading. Perfectly rational risk-neutral traders with common knowledge of the market structure backward induct, which prevents bubbles from happening (c. conjecture 1). In order to understand these trades from a 'rational' modeling perspective, we have to assume either risk-aversion or a more complex - and hence ad hoc - model structure (such as the expectation of capital gains, or boundedly rational players that have computational limits, c. Rubinstein (1998)).

Porter, Smith (1995) have shown that risk-aversion alone does not account for what are errors in decision making for risk-neutral players with common knowledge (trades at h). On the other hand, Lei, Noussair, Plott (2001) have argued that lack of common knowledge of rationality alone does not explain the trading behavior either. What about computational limits for the players? Were they to account for a majority of non-theory conform trades, we would see the following two qualitative patterns:

1. The number of trades at more than UBV per period would go down over time.
2. The share of 'rational' trades would go up over time.

Figure 9 shows that the first is very much not the case in our experiment - in fact, if anything we observe the opposite. We can get a rough estimate of the level of theory-conform trading from our noise trading model. While we do find

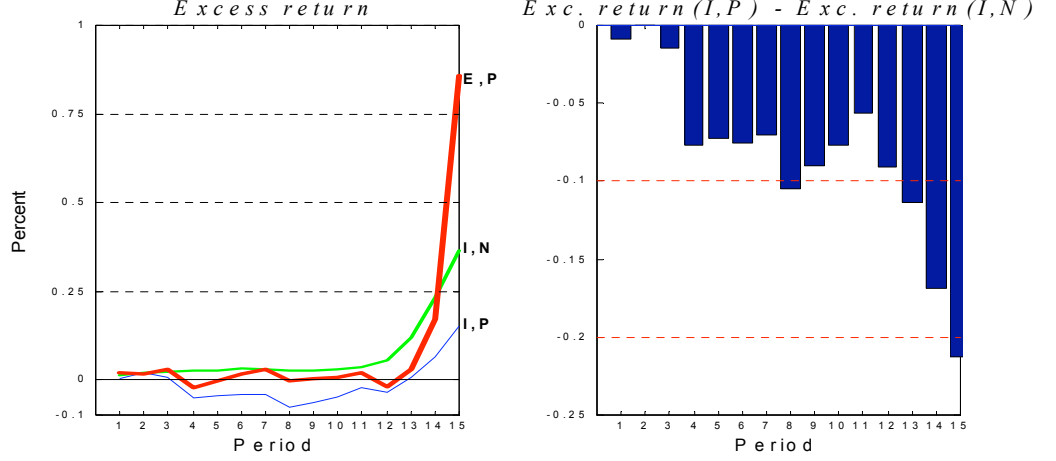


Figure 13: Dividend excess return

the second pattern in (I,N) experiments (and approximately in (E,P) experiments (figure 11), the implied proportion of rational traders in (I,P) treatments is highly erratic - no systematic pattern exists. Note that all three intervention periods - periods 4, 8, and 12 - are local minima of rationality. But it is not only the pattern (or lack thereof) of the evolution of the implied share of rational traders over time that is interesting: except for in one or two periods, the proportion of rational traders is higher in control than in policy treatments (figure 12). We conclude that conjecture 3 conforms well with the data.

Conjecture 4: We have remarked above that it is clear from figure 6 and table 7 that higher interest rates create additional income (investing into bonds is more profitable); figure 13 shows that higher interest rates also increase the opportunity costs of holding stock. Given that the relative value of stocks per period is lower in (I,P) experiments, participants should hold more bonds as a share of their portfolio. On the other hand, because other factors - such as liquidity - influence investment decisions too, and because total income is higher in policy treatments, we do not a priori know whether to expect absolutely less money in their trade accounts. Figure 7 shows that both are true, table 7 that the difference is highly significant (a little stronger, surprisingly, for absolute than for relative differences). The change is unanimous: the variance of the share invested in bonds goes down significantly from (I,N) to (I,P) experiments (figure 8 and table 7). Experienced subjects recognize their opportunities better (figures 7 and 8 and table 7).

Table 8 shows the value impact on Walrasian price adjustment.

In inexperienced policy sessions, the price process recognizes an increase in interest rates. Note that this is less the case in experienced sessions. We think that this is because participants who faced the *exact* same market situation

Variable	Treatment							
	I,P		I,N		E,P		E,N	
	Rho	p-value	Rho	p-value	Rho	p-value	Rho	p-value
Excess bids	0.451	(<10 ⁻⁴)	0.356	(0.001)	0.515	(0.001)	0.278	(0.336)
Volume	0.086	(0.438)	-0.050	(0.655)	0.057	(0.718)	-0.156	(0.597)
Total liquidity	-0.221	(0.043)	-0.124	(0.262)	0.099	(0.532)	0.046	(0.876)
Trade liquidity	-0.289	(0.008)	0.269	(0.014)	0.135	(0.395)	0.328	(0.253)
Change of i	-0.230	(0.035)	n/a	(n/a)	-0.154	(0.331)	n/a	(n/a)
Div. excess return	-0.314	(0.004)	0.055	(0.618)	0.530	(<10 ⁻⁴)	-0.209	(0.474)

Table 8: Spearman's Rho between price change and other variables

before anticipated changes in interest rates. If they did, prices should have reflected their expectation even before the change actually hit the market. The same table shows how changes in the dividend/excess return¹⁵ influence market prices. Because we held the interest rate constant in control experiments, there is no significant impact of the dividend/excess ratio in (I,N) and (E,N) sessions.

We have anecdotal evidence of the importance of strategic investment decisions that are far from what the APH would predict. The asset market is a zero-sum game, so the optimal decision on a group level would have been investment in bonds (money in trade accounts is - on a group level - money lost because it does not carry interest). In other words, it would have been 'rational' for the group to keep the stock and invest all cash in bonds, or from the viewpoint of the individual participant to sell the stock for a 'good' price and invest the proceeds in bonds. We identified our best two traders. In their (I,P) sessions, they behaved exactly in this fashion: they sold all their stock for m and h prices early on, invested the money in bonds, and leaned back for the rest of the experiment. As a result, over two rounds, they made about six times as much money as the worst participant in their group.

Figure 10 charts the success dependent spread: it increases from (I,N) to (I,P) treatments (the increase is statistically highly significant).

Summing up, we have ample evidence to support conjecture 4.

Conjecture 5: We have pointed out before how conjectures 3 and 4 put the APH in doubt. Consider additionally the following: we held the experienced sessions right after the inexperienced sessions, on the same day. If the experimental subjects actually traded because they were bored, the market volume (turnover) would not be significantly different in the inexperienced and the experienced sessions. The same applies if they traded because they (wrongly) assumed that we wanted them to trade. Table 6, however, shows that turnover

¹⁵The dividend/excess return is defined as

$$d-e_t = \frac{E[d_t]}{P_{t-1}} - i_t.$$

More formally, P_{t-1} in the denominator should be the conditional price expectation in period $t-1$ for period t . Our formula amounts to assuming static expectations.

goes down from (I,P) to (E,P) sessions (the difference is highly significant). As we have pointed out before, it is the profit-maximizing strategy to invest all money in bonds. If rational traders understand this, or learn it with experience, turnover goes down. Hence, we interpret the decline in trade volume to signal that participants learned to recognize value better in experienced session. We have another hint that trade is not due to the APH. If trade were due to the APH, we were likely to see investment patterns such as

1. Constant share of the participants' portfolio invested in bonds over time.
2. Constant amount of trade liquidity on the participants' trade account over time,

in particular the second. We see neither pattern in our experiments (figures 7 and 8, and table 6).

4 Conclusions

The first conclusion follows immediately from the discussion of conjecture 1 in section 3.5.

Conclusion 1 *The data of our experiments contradict the predictions of standard rationality models of economic theory.*

Conjectures 3 and 4 imply another conclusion:

Conclusion 2 *Interest policy is modestly beneficial for the size and duration of bubbles. On the other hand, it strongly increases the volatility in the stock market.*

Conjecture 4 in conjunction with conjecture 3 also supports another corollary:

Conclusion 3 *The stock market is characterized by departures from trading at values predicted by basic economic models. The portfolio decision more general, however, is consistent with rational choice.*

In sum, we conclude that while it is good to be the king (or Alan Greenspan), it is not easy. When even in a modestly complex setting (such as in our experiment) it is hard to calibrate an interest policy that deflates bubbles in a timely and controlled manner - how much more so must this have been the case in the American economy? We have some success, but find ourselves trading-off the deflation of a bubble with an increase in market volatility, a certainly undesirable by-product. On the other hand, it is satisfying to see how the rationality of people shines through their portfolio choice. For real stock markets, it stands to hope that this self-motivation of value-driven people, together with timely interventions of central banks, will conceivably manage to do just as well, and hopefully better, than our experiment implies in a laboratory setting.

5 Appendix

5.1 Figures and tables by experiment

We first show mean contract prices, trade volume and fundamental value for generic experiments with inexperienced and experienced traders. In the inexperienced case, we held two sessions at the same time. Both groups faced the same dividend payments. One was subjected to interested policy; we fixed the interest rate at 5 percent for the other. This is why we also show the results of these two groups in the same graph.

We next plot the bubbles for all generic experiments. For definitions of the 'naive' and 'sophisticated' perspective, check section 3.1.

We then present the same figures for non-generic experiments. The section concludes with a table of the bubble measures by experiment.

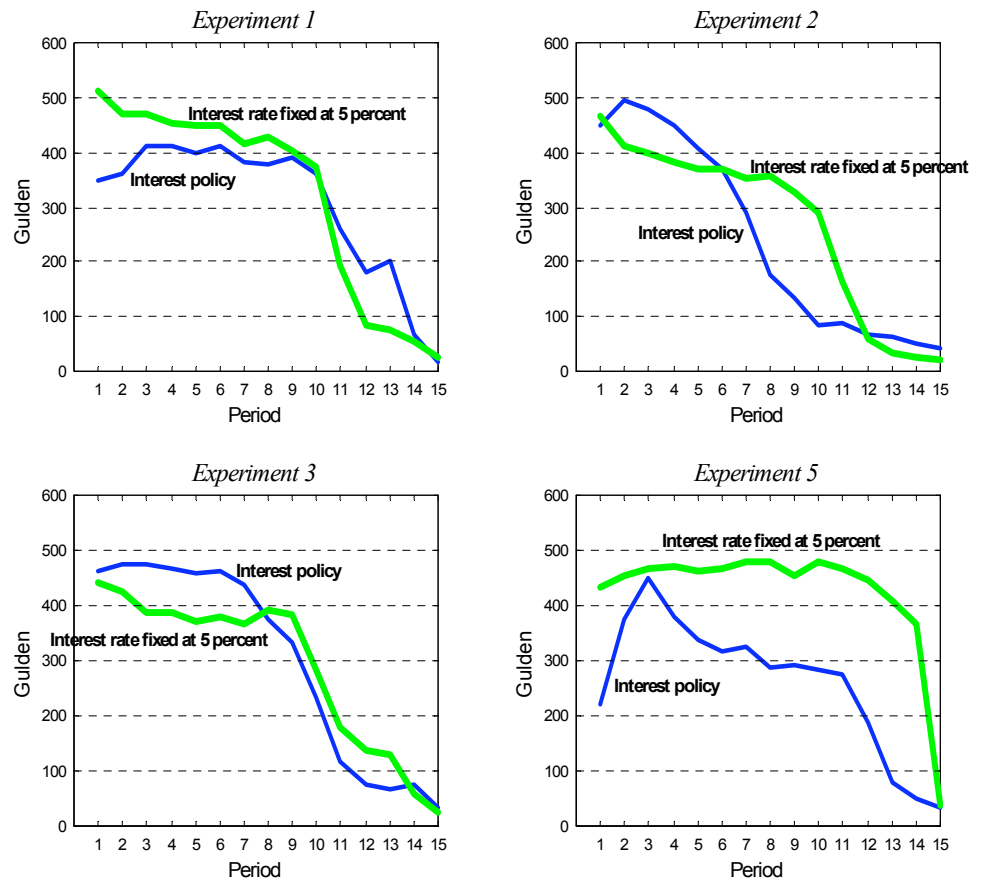


Figure 14: Mean contract prices in experiments 1, 2, 3 and 5

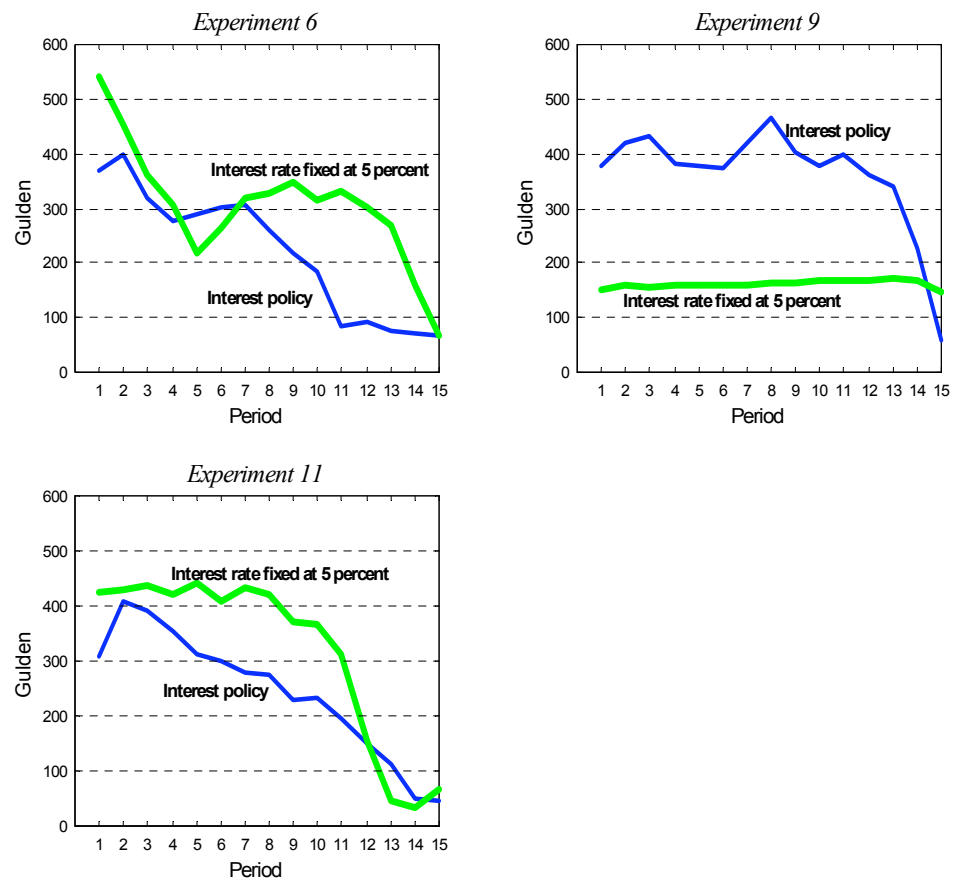


Figure 15: Mean contract prices in experiments 6, 9 and 11

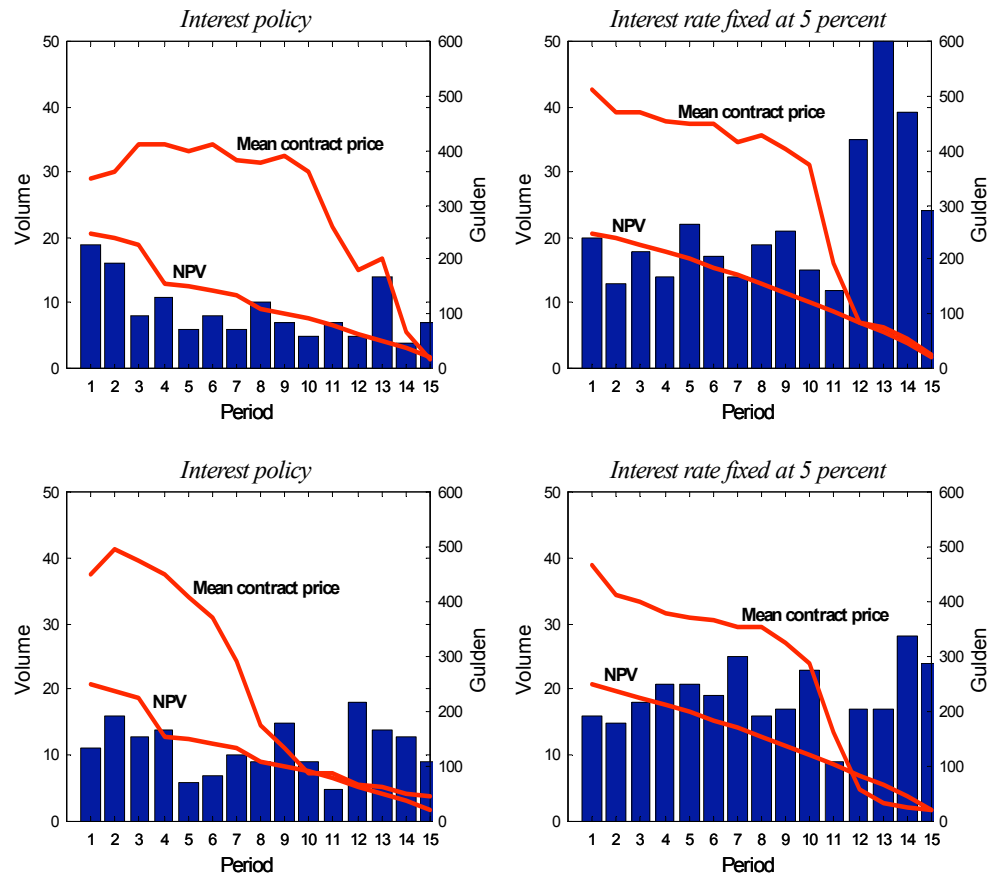


Figure 16: Experiments 1 (top row) and 2 (bottom row) - Price and volume

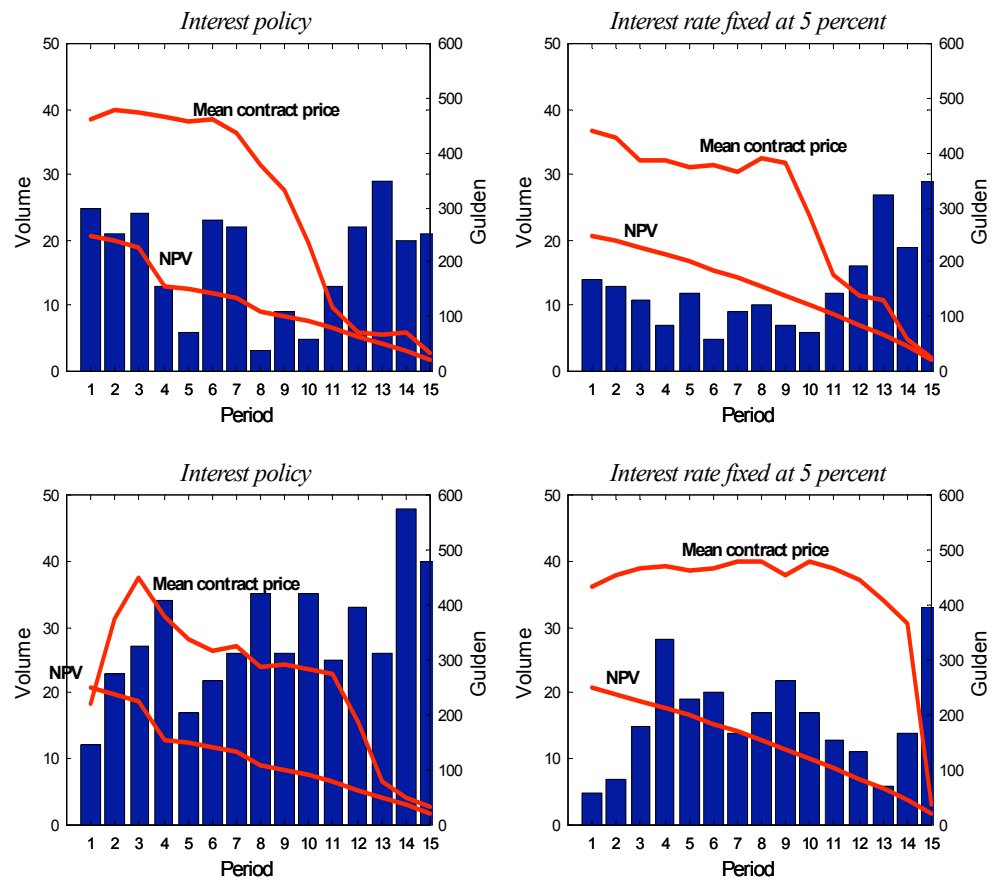


Figure 17: Experiments 3 (top row) and 5 (bottom row) - Price and volume

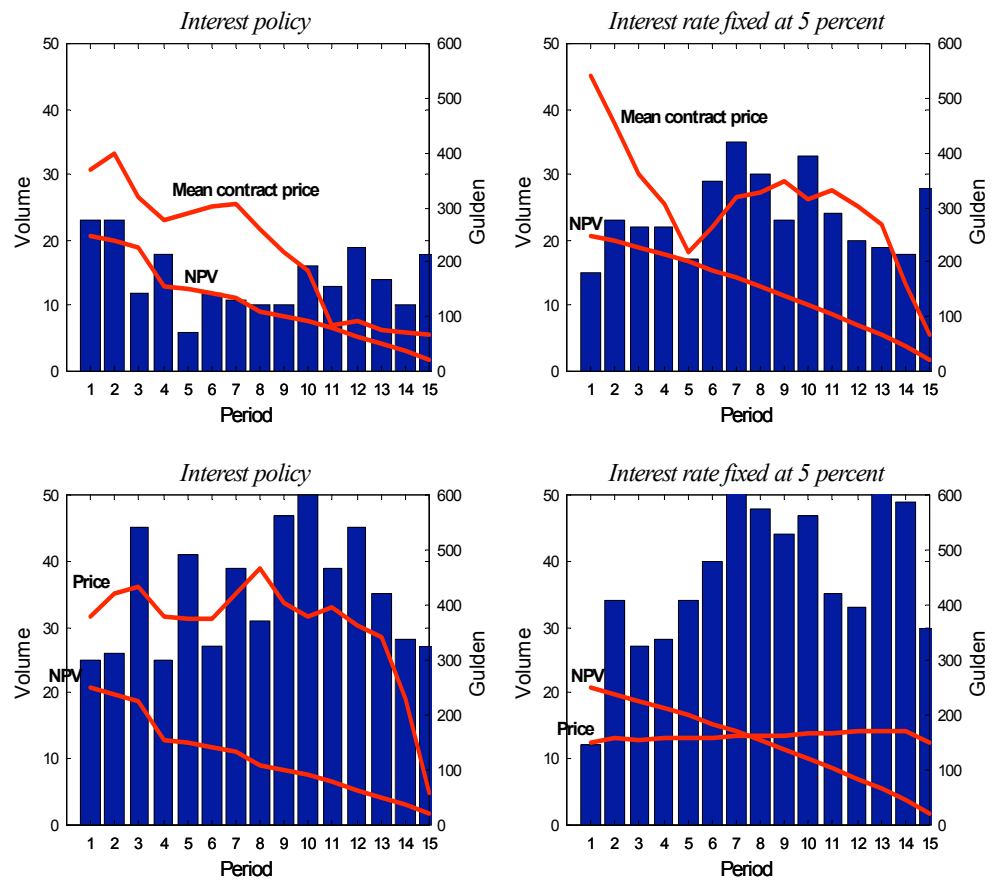


Figure 18: Experiments 6 (top row) and 9 (bottom row) - Price and volume

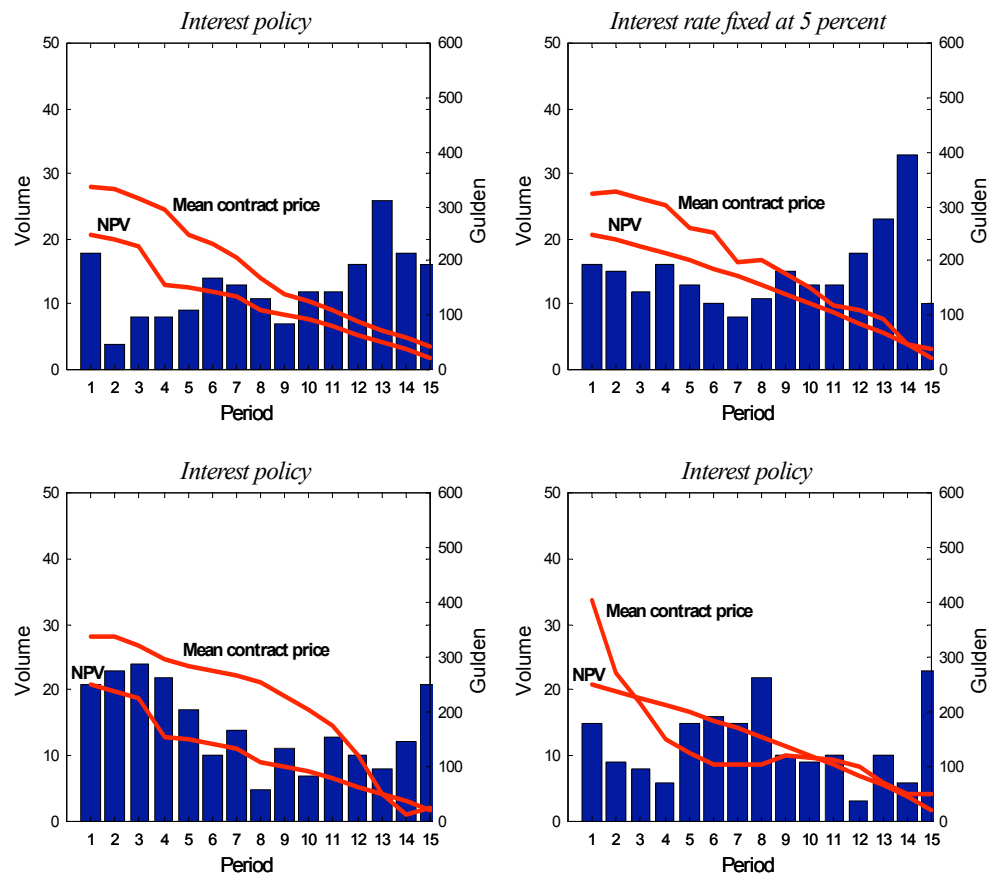


Figure 19: Experienced experiments 4 (top left), 7 (top right), 10 (bottom left) and 12 (bottom right) - Price and volume

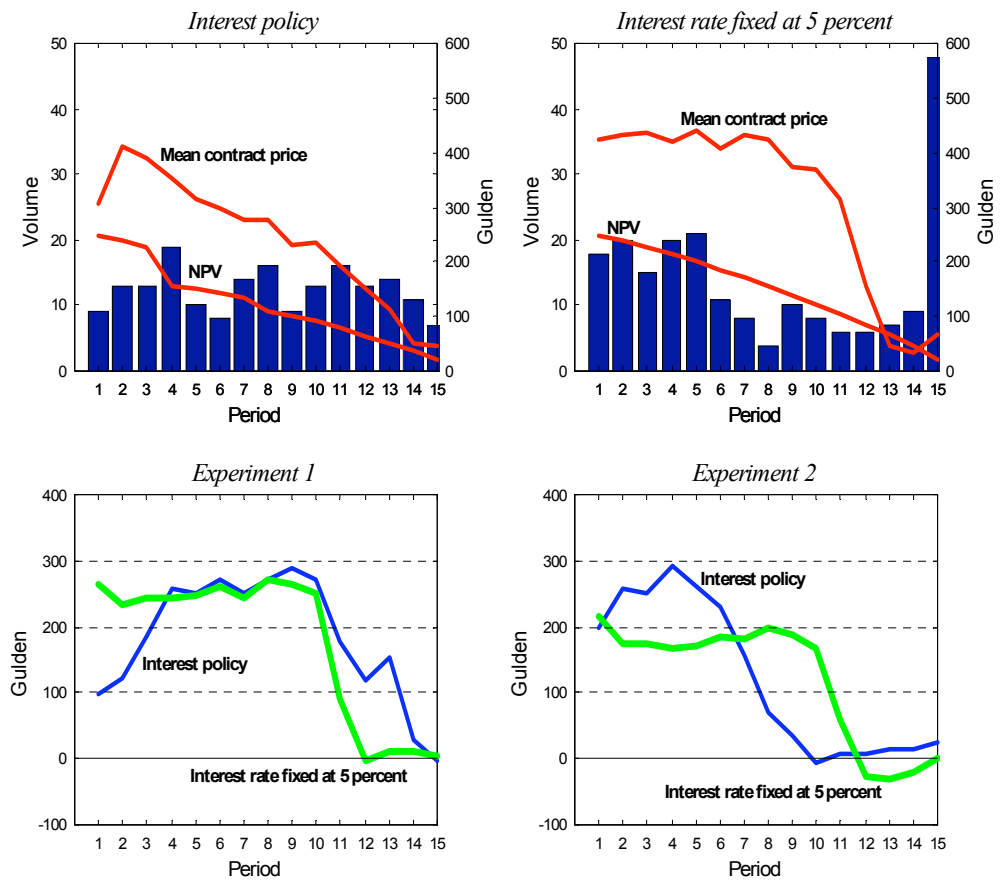


Figure 20: Experiment 11 - Price and volume (top row), and sophisticated bubbles in experiments 1 and 2 (bottom row)

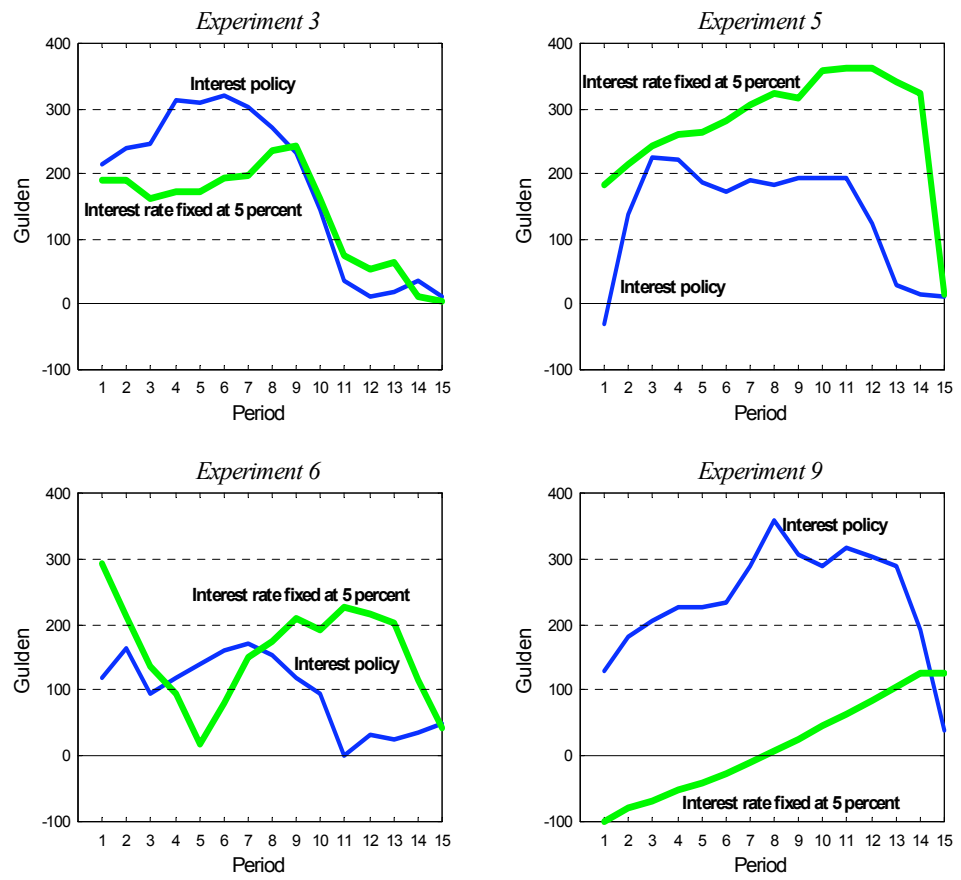


Figure 21: Sophisticated bubbles in experiments 3, 5, 6 and 9

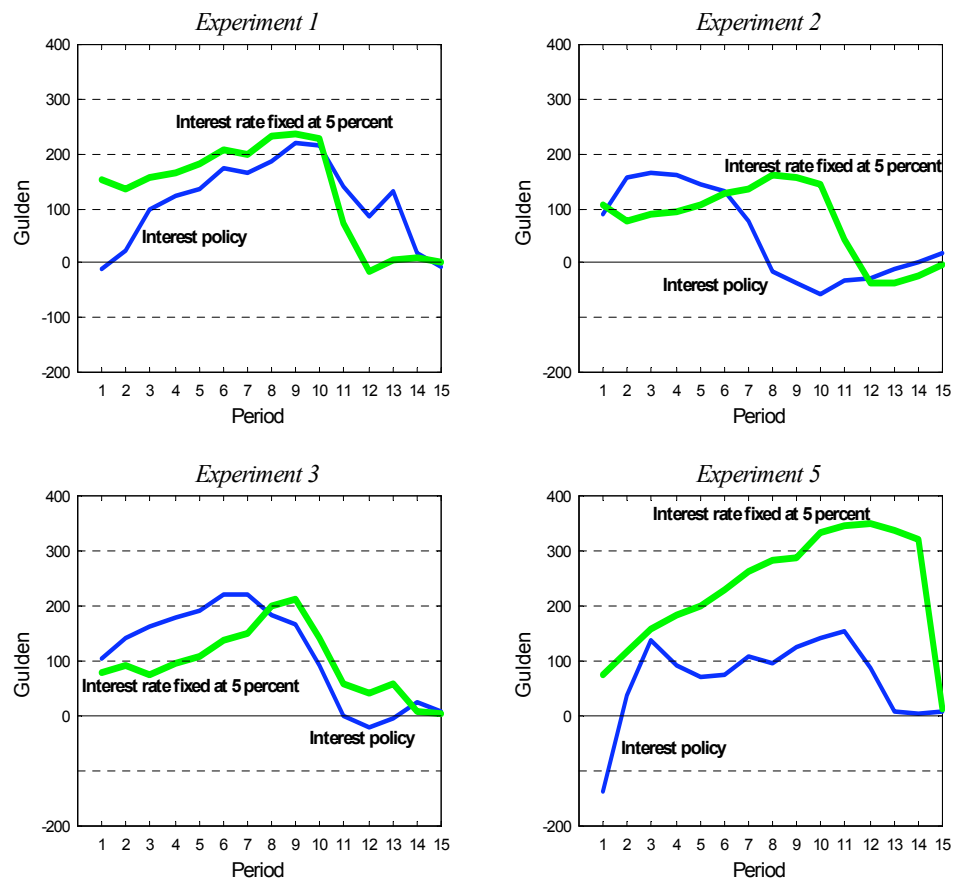


Figure 22: Naive bubbles in experiments 1, 2, 3 and 5

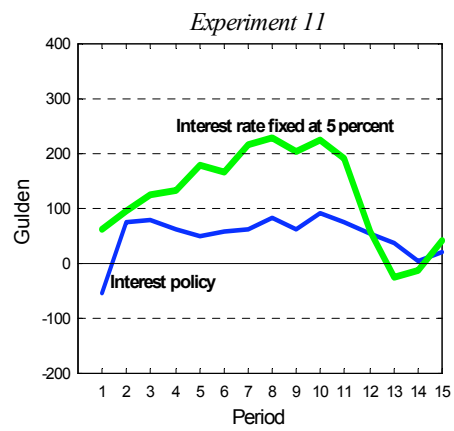
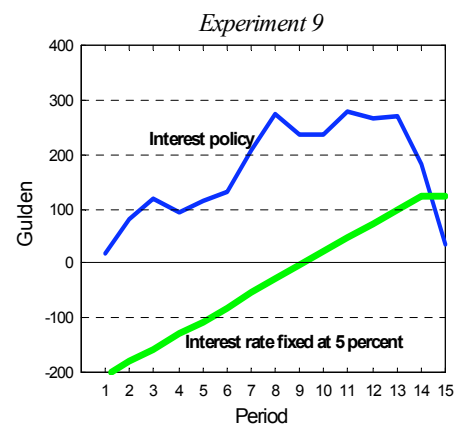
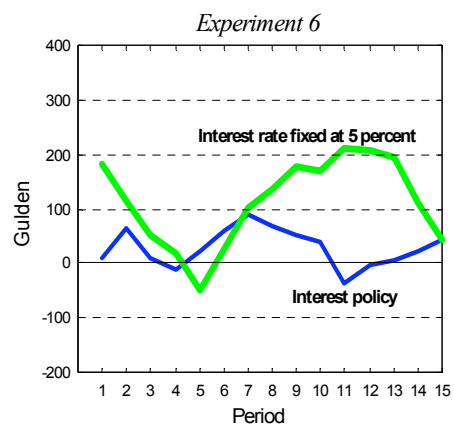


Figure 23: Naive bubbles in experiments 6, 9 and 11

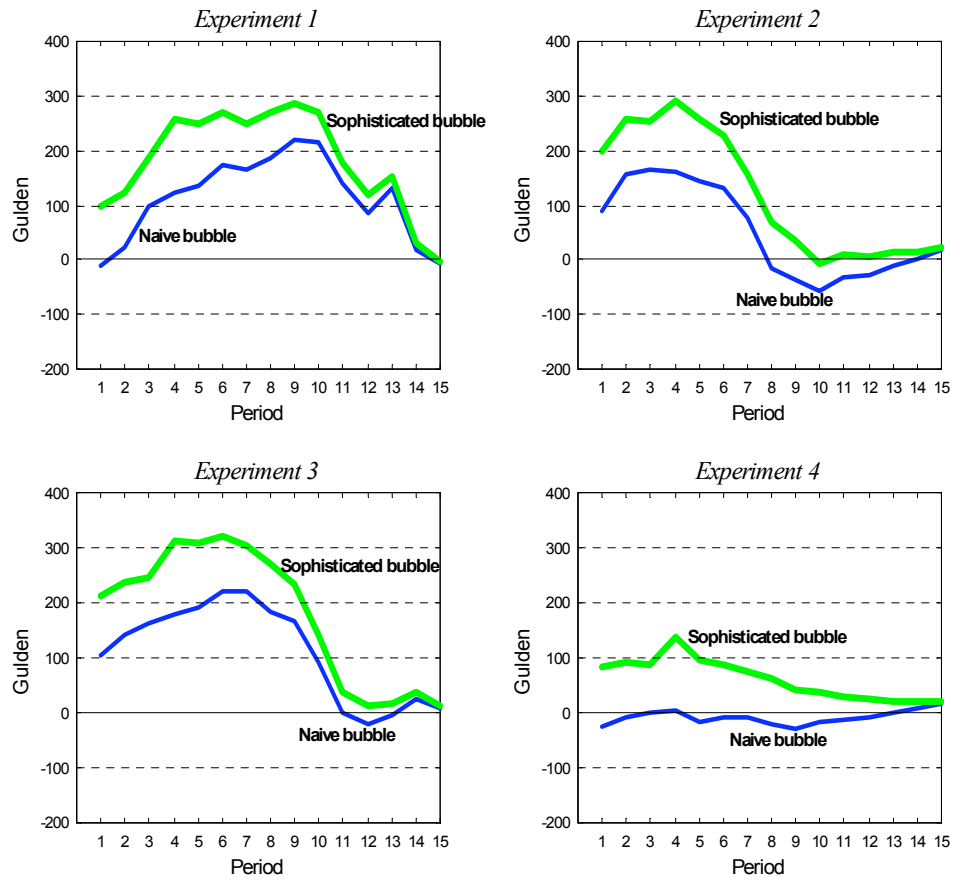


Figure 24: Comparison of bubble measures in generic experiments with interest policy (I)

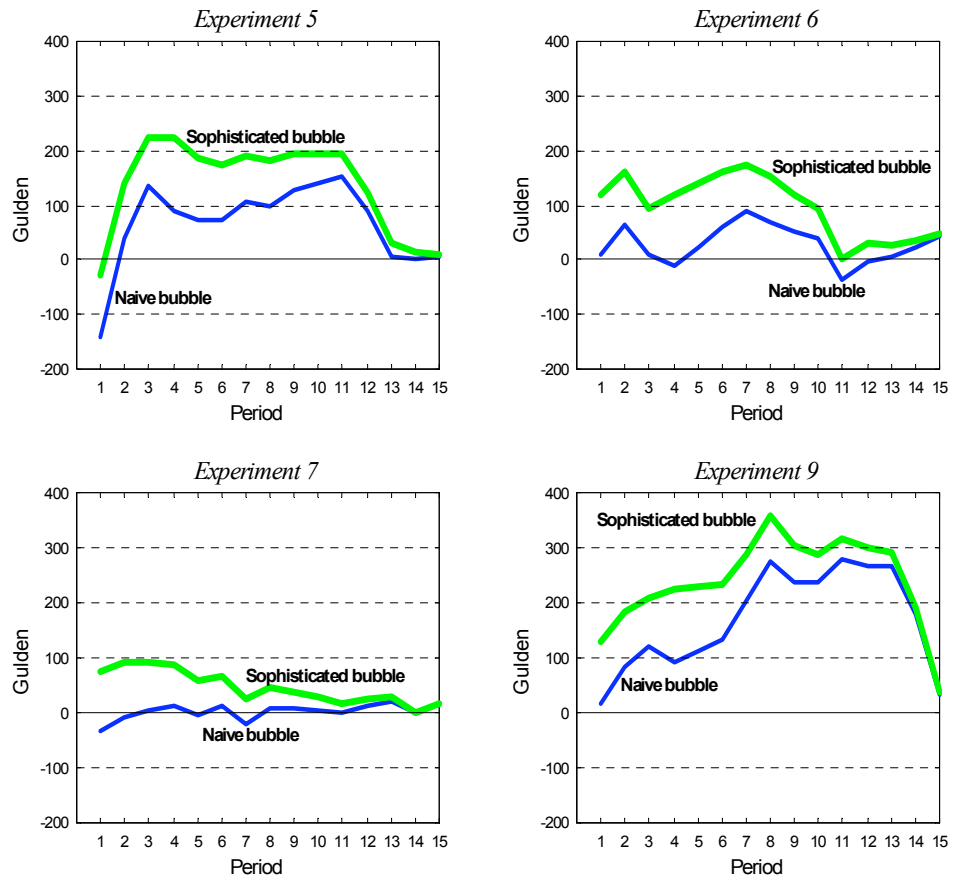


Figure 25: Comparison of bubble measures in generic experiments with interest policy (II)

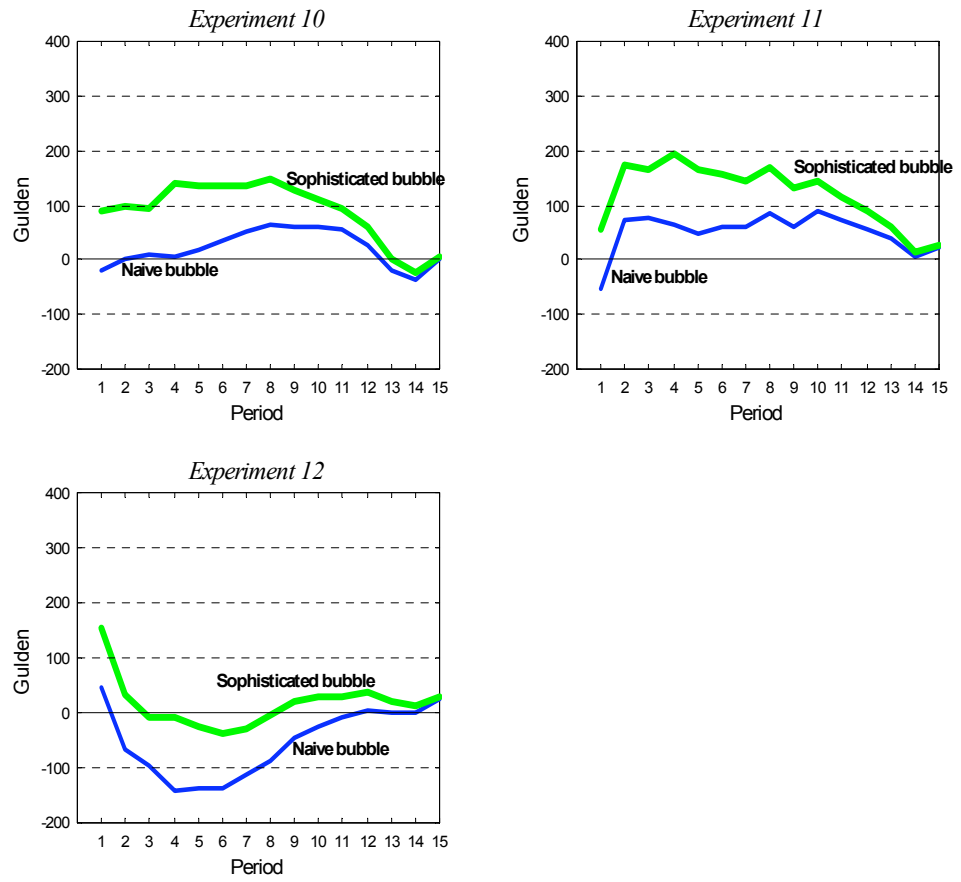


Figure 26: Comparison of bubble measures in generic experiments with interest policy (III)

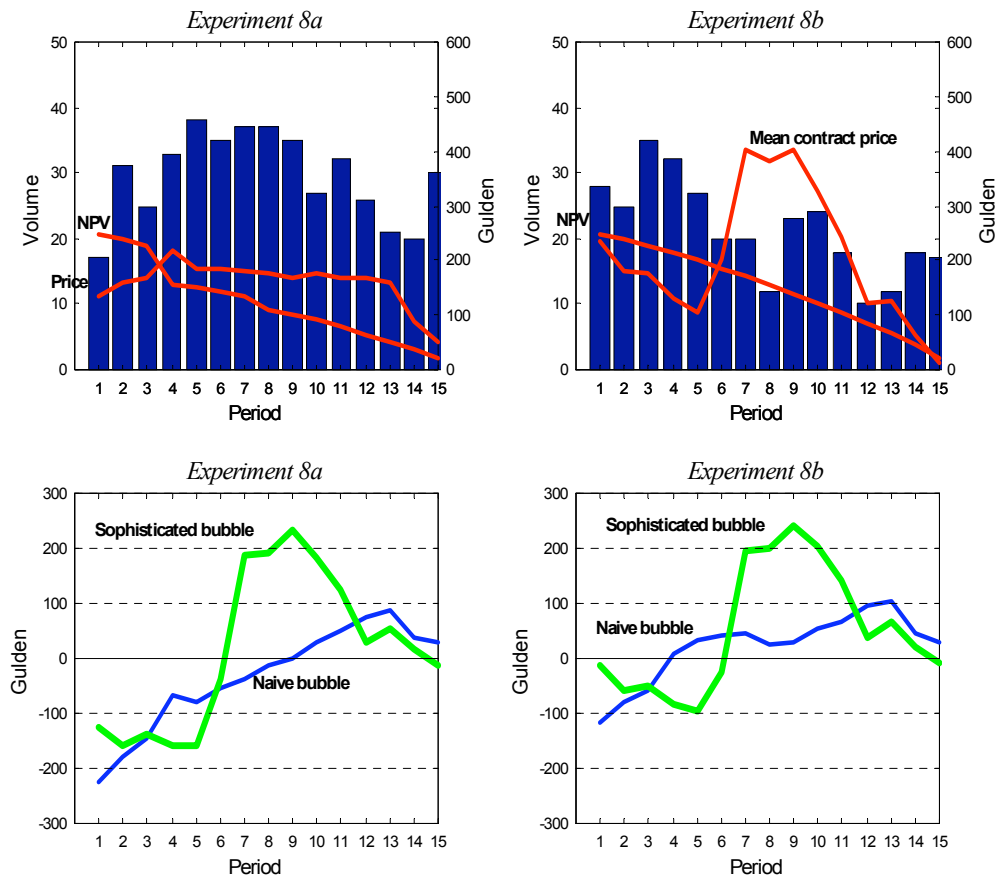


Figure 27: Mean contract prices, trade volume, and bubbles in non-generic experiments

Treat- ment	Bubble measure						
	Dura- tion	Turn- over	Ampli- tude	Vola- tility	Devi- ation(s)	Devi- ation(n)	Rel. bubble
I,P	3	2.11	1.20	0.14	1.52	0.94	0.9
	3	3.20	1.23	0.18	2.25	1.43	0.53
	2	5.38	1.01	0.39	1.76	1.07	0.51
	4	2.69	0.69	0.15	1.23	0.46	0.44
	7	6.63	1.29	0.29	2.98	2.12	0.67
	1	2.64	0.72	0.22	1.72	0.82	0.50
I,N	2	3.58	0.99	0.14	1.64	1.12	0.50
	6	2.46	0.96	0.14	1.77	1.22	0.45
	7	3.01	1.39	0.08	3.46	2.91	0.64
	4	4.48	1.10	0.28	1.97	1.50	0.52
	14	7.23	0.91	0.05	0.80	1.20	0.40
	2	3.01	1.15	0.16	2.50	1.88	0.53
E,P	1	2.40	0.47	0.10	0.77	0.16	0.34
	6	2.73	0.70	0.12	1.17	0.39	0.55
	2	2.53	0.77	0.12	0.46	0.89	0.25
E,N	2	2.83	0.36	0.10	0.58	0.15	0.23

Table 9: Bubbles measures by experiment

5.2 The interest rate policy algorithm

The algorithm is written in a pseudo code that resembles *C*. Knowledge of any programming language should be sufficient to read our code.

Pseudo code for interest rate policy

(define variables)

```

float X [15]           /* mean contract prices */
float FV [15]          /* fundamental values */
float b                /* bubble tolerance,  $b \in (0, 1)$  */
float irange [5]       /* possible interest rates */
integer pointer        /* pointer to current interest rate */
float i               /* current interest rate */
integer time           /* keep track of last intervention */
Boolean posbub        /* indicator function for positive bubble */

```

```

Boolean negbub          /* indicator function for negative bubble */

(initialize parameters)

b = 0.5
irange [1] = 0.01      /* decide on possible interest policy */
irange [2] = 0.05
irange [3] = 0.11
irange [4] = 0.15
irange [5] = 0.21
pointer = 2            /* begin with i = 0.05 */
i = irange [pointer]
time = 1              /* first intervention in period 4 (=1+3) */
posbub = False       /* no bubbles yet */
negbub = False

(subroutine raise interest rates)

begin subroutine HIGHER
  if pointer ≤ 4
    pointer = pointer + 1
    time = 0          /* intervention has happened
                      so no more interv. for 3 periods */
  endif
end subroutine

(subroutine lower interest rates)

begin subroutine LOWER
  if pointer ≥ 2
    pointer = pointer - 1
    time = 0          /* intervention has happened
                      so no more interv. for 3 periods */
  endif
end subroutine

(main program)

begin MAIN
for t = 1 to 15
  read X [t]          /* get current values */
  read FV [t]
  time = time + 1      /* time since last
                      intervention has passed */

```

```

(check all bubble states and adjust)

if  $X[t] \geq (1 + b)FV[t]$  /* check for pos. bubble */
     $posbub = True$  /* positive bubble */
     $negbub = False$ 
    if  $time \geq 4$  /* at least 4 periods since
                    last intervention */
        subroutine HIGHER
    endif
endif

if  $X[t] \leq (1 - b)FV[t]$  /* check for neg. bubble */
     $negbub = True$  /* negative bubble */
     $posbub = False$ 
    if  $time \geq 4$  /* at least 4 periods since
                    last intervention */
        subroutine LOWER
    endif
endif

if ( $posbub = True$  and  $X[t] \geq (1 - b)FV[t]$  and  $X[t] \leq FV[t]$ )
     $posbub = False$  /* was pos. bubble -> no bubble */
    subroutine LOWER
endif

if ( $negbub = True$  and  $X[t] \leq (1 + b)FV[t]$  and  $X[t] \geq FV[t]$ )
     $negbub = False$  /* was neg. bubble -> no bubble */
    subroutine HIGHER
endif

 $i = irange[pointer]$  /* adjust interest rate */
next  $t$  /* end of this period */

end MAIN

```

5.3 Experimental instructions

Overview¹⁶

You are now participating at an economic experiment that deals with trading in stock markets. Contingent on your decisions in this experiment, you can earn money in excess of your participation fee of 10 Franken¹⁷. Hence, it is important that you read these instructions very carefully. At the end of the document you find some questions. Please answer them and tell us when you are done.

Please refrain from talking for the duration of the experiment. If you have questions, please ask us. If you do not observe this rule, we will have to exclude you from this experiment and all payments, and ask you to leave.

The experiment consists of 15 periods. The currency of the experiment is called Gulden¹⁸, not Franken. You can earn Gulden in each period. We will exchange your Gulden to Franken at the end of the experiment, at a rate of

$$1000 \text{ Gulden} = 120 \text{ Rappen}^{19}$$

Basic structure of the experiment

This experiment is about investment of money. You can buy either stocks or bonds. You can also trade in stocks. Bonds bear interest. Money you use for trade does not bear interest. Each period, stocks pay a dividend.

At the beginning of the experiment, i.e. at the beginning of the first period, you receive an endowment in money and in stocks. Each period is structured in the same way. You first decide how much money to put into bonds and how much money to reserve for trade in stocks. You can then trade in stocks, i.e. sell them to other participants or buy them from other participants. You can only use the money you reserved at the beginning of the period for trade in stocks. After the trade phase, you receive a dividend for each stock in your possession, and interest on your bonds. You can use this money and the stocks again in the next period. Some details:

1. **The dividend:** Each stock pays a dividend at the end of every period. The dividend amount is determined by chance. It is either 0, 8, 28 or 60 Gulden for every stock in your possession at the end of the respective period. Each amount is equally likely and determined in each period with the aid of a dice. In other words, on 'average' (over many periods) you can expect to earn 24 Gulden per period per stock in your possession, if you are lucky 60, and if you are unlucky 0.
2. **The interest:** You receive interest on money invested in bonds. The interest rate is 5 % per period originally. It is variable which means that

¹⁶The instructions are translated from German.

¹⁷One dollar are about 1.8 Franken.

¹⁸A Gulden is a medieval coin.

¹⁹100 Rappen = 1 Franken.

it is possible, but not certain that the interest rate is going to change in later periods. You do not receive interest for money on your trade account.

The accounts

1. **Stock account:** At every point in time, this account shows the current number of stocks in your possession.
2. **Cash accounts:**
 - (a) **Trade account:** You can use the money on this account to buy stocks during the trade phase (c. **periods**). It does **not** bear interest.
 - (b) **Bond account:** This account contains the money that you declared as non-trade money for this period. It **does** bear interest.
 - (c) **Total cash:** The sum of the previous two accounts.

Your profit

It is very easy to calculate your profit in Gulden (in addition to the 10 Franken showup fee). It is:

Money on your total cash account at the end of period 15.

You do not receive anything for stock in your stock account at the end of the experiment. During the experiment, you have the following options to make a profit:

1. Buying and selling of stocks
2. Dividends on your stocks
3. Interest on cash in bonds

The periods

1. **Splitting your cash on the accounts**

At the beginning of each period, you receive an overview over the current state of your wealth (see above). This overview specifies:

- The average price at which stocks traded in each of the previous periods.
- The state of your total cash account and your stock account.
- The current interest rate that will be paid in this period on money in your bond account.

On the bottom of the page you notice the icon of a calculator. When you click it, a calculator appears on your screen. You can use it for calculations at this stage of each period.

You then have to make a decision before trade in stocks begins:

- Divide your cash between your trade account and your bond account. In the following trade phase of this period, you can use **only money in your trade account** to deal in stocks. You receive interest on money in your bond account. We added a button 'calculate interest' to help you translate percentage points into Gulden. When you hit this button you see, under the bond account, the amount in Gulden you would receive at the end of this period at the current interest rate if you put as much money into the bond account as you currently do. You can try out different amounts in your bond account and compare the Gulden they pay you at the end of the period before you continue.
- When you are happy with how you split your cash on the two accounts, first press the 'calculate interest' button and then the 'ok'

button on your screen. Even if you want to pass on the calculation of interest, you have to first hit the 'calculate interest' button and only after that the 'ok' button. The experiment switches to the (stock) trade phase of this period once the last participant has hit the 'ok'-button.

2. The (stock-) trade phase

In each period you have 2 minutes and 30 seconds to trade stocks. Check the trade screen below. On top is the current period and time remaining. In the middle of the screen, you see the number of stocks in your stock account and the Gulden in your trade account.

In the lower part of the screen, you trade:

- (a) You make **sales offers** to the other participants in the window on the very left. Enter the price you are asking for in the blue field and press 'sell'. This price appears then on the screen of all participants right next to this field, in the field 'sales offers.' You can only enter integer, positive amounts, and your offer must be lower than the currently lowest offer.

Period		1 of 15		Remaining time [sec]: 146	
<div>Period</div> <div>Price</div>					
<div>Trade account ...</div> <div>Stocks ...</div>					
<div>Your offer to sell</div> <input type="text"/> <div>Ask</div>	<div>Offers to sell</div> <div>Buy</div>	<div>Trade prices</div> <div></div>	<div>Offers to buy</div> <div>Sell</div>	<div>Your offer to buy</div> <input type="text"/> <div>Offer</div>	

- (b) The next window contains the sales offers of *all* participants. You can buy **one** stock at one of these prices. The currently best offer is highlighted. When you hit 'buy', you automatically buy a stock from the participant who made this offer. The respective amount is debited to your **trade account**.
- (c) The window in the center of the lower part of the screen lists all prices at which stocks were traded in this period.

- (d) The fourth window contains the price bids of *all* participants. You can **sell** one of your stocks at one of these prices. The best bid is highlighted. When you hit 'sell', you sell one of your stocks to the participant who made this offer. The resulting cash amount is credited to your **trade account**.
- (e) You can make an **offer to buy** in the window on the very right. Enter the amount at which you are willing to buy a stock into the blue field and hit 'buy'. This price subsequently appears on all screens in the field '**offers to buy**.' You can only enter integer, positive amounts, and your offer must be higher than the currently highest offer.

Some **trade rules for stocks**:

- Do not sell stocks that you do not own yet.
- Do not sell stocks to yourself.
- Do not buy stocks with debt, i.e. you are not allowed to offer more for a stock than you currently have on your *trade account* (You cannot access money in your bond account for trade in this period).

The computer will enforce these rules automatically. If ever you are astonished about problems with the execution of one of your orders, please check first whether you followed these rules.

3. Summary of this period

Period		1 of 15																	
<table> <tr> <td>Money in bonds</td> <td>...</td> </tr> <tr> <td>Revenue on money in bonds (interest-%)</td> <td>...</td> </tr> <tr> <td>Trade account</td> <td>...</td> </tr> <tr> <td>Dividend per stock</td> <td>...</td> </tr> <tr> <td>Number of stocks</td> <td>...</td> </tr> <tr> <td>Revenue from dividends</td> <td>...</td> </tr> <tr> <td>New total cash</td> <td>...</td> </tr> <tr> <td>Number of stocks</td> <td>...</td> </tr> </table>				Money in bonds	...	Revenue on money in bonds (interest-%)	...	Trade account	...	Dividend per stock	...	Number of stocks	...	Revenue from dividends	...	New total cash	...	Number of stocks	...
Money in bonds	...																		
Revenue on money in bonds (interest-%)	...																		
Trade account	...																		
Dividend per stock	...																		
Number of stocks	...																		
Revenue from dividends	...																		
New total cash	...																		
Number of stocks	...																		
			continue																

At the end of each period, you receive a summary of your profits from dividends on your stock and interest on money in your bond account.

Additionally, this summary shows the current state of your accounts. Press the 'continue' button once you are ready. Check the screen shot and the short description under it.

Line 1: The money you put into your bond account at the beginning of this period.

Line 2: Your profit in Gulden from the interest on the amount from line 1.

Line 3: Cash on your trade account at the end of this period, i.e. *after* the stock trade phase.

Line 4: This period's dividend (per stock).

Line 5: The number of stocks you own at the end of this period, i.e. *after* the stock trade phase.

Line 6: The product of lines 4 and 5.

Line 7: The sum of lines 1, 2, 3 and 6.²⁰

²⁰The participants were then asked to answer several questions that followed the instructions. Their sole purpose was to make sure that the participants had correctly understood the instructions.

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